THE QUARTERLY

JOURNAL OF SCIENCE.

APRIL, 1878.

I. ECONOMY OF NITROGEN.

WE have repeatedly heard the observation that chemists in complaining of "waste" are guilty of inconsistency. If matter, it was argued, is indestructible, and if no element can be converted—whether designedly or accidentally—into another, waste is simply an impossibility. Such censures are the natural result of an education based rather upon Literature than upon Science, and are in themselves of no moment. Still, as in England—unlike Germany—a man of general "culture" is considered entitled to lay down the law upon any subject whatsoever, and is often listened to with more attention than the specialist or the man of original research, it may be useful to point out the fallacy involved.

Waste, from a chemico-technical point of view, depends not on the fancied destruction of some element, nor on its transmutation into some other simple body, but on useful matter being thrown into a state where it is no longer immediately available for our purposes. It is thus, so to speak, locked up; withdrawn, for a longer or shorter time, from circulation, just like specie buried by a miser of the old school. Such transmutations may take place in various manners. A useful substance, whether simple or compound, may be brought into the state of a highly dilute solution, or may be mixed with solid matter in so minute a proportion as to be practically irrecoverable. Nature presents us with many instances of bodies, valuable, indeed, in themselves, but rendered useless by admixture with a vast excess of alien substances. We need only mention, as cases in point, the silver and iodine, whose presence has been demonstrated

in sea-water, the gold in many varieties of quartz and pyrites, and the sulphur in coal. To obtain a shilling's worth of silver from sea-water costs more than a shilling, and the amount of precious metal thus dissolved in the ocean—immense as it doubtless is—may be, in our present state of knowledge, pronounced practically non-existent.

Now Art is constantly engaged in reducing useful substances into a similar condition. Every time a sovereign is handled in the course of business, or shaken against other coins in the purse or the cash-box, some small trace of gold is abraded. The quantity lost may be too trifling to be visible, and might even elude spectroscopic detection; but that loss does constantly take place we know, since by the mere continuance of such ordinary wear and tear the current coin of the realm ultimately becomes "light." The silver fork or spoon, the brass-work of the microscope and the balance, the leaden gutter or spout, the iron key, spade, or wheel-tire, are all in like manner gradually wearing away, losing their substance in particles so minute as to elude observation, and becoming thus distributed no one can say where. Nothing is indeed destroyed, but everything is becoming mixed. There may be natural processes by which all these microscopic films and fragments of metals, and other useful substances, are being sorted out and collected together, like to like, and are being formed into veins and beds of ore, &c., for future generations to extract and again bring into use; but such processes, if they exist at all, are assuredly very slow. We scatter more rapidly than Nature gathers. We are living beyond our income, and, except as far as some few of the commoner elements are concerned, we must ultimately, in the words of the old adage, "find the bottom of the bag."

Another form of waste consists in taking an elementary substance, useful in its free state or in some particular combination, and transmuting it into compounds no longer capable of the same applications. Of this class of waste the consumption of coal may serve as the type. Carbon and certain compounds of carbon and hydrogen combine with oxygen, the main resulting product being carbonic acid, accompanied, in the case of the hydrocarbons, with watery vapour. Now carbonic acid is certainly not useless in the economy of Nature, being one of the most important ingredients in the food of plants, but in our hands it is what the old alchemists called a *caput mortuum*—a residue which, economically speaking, must be pronounced intractable. There is no need to insist here upon the vast supply of this

gas poured daily into the atmosphere from the combustion of coal and other fuel, from the respiration of animals, and from the fermentation and putrefaction of organic matter,—nor to show how during all these processes carbon, either free or existing in some organic combination, is being transformed into carbonic acid at an astonishing rate. That at the same time the carbonic acid of the atmosphere is being decomposed by growing vegetation, its oxygen set free, and its carbon assimilated,—re-converted into organic compounds,—is not capable of question. But is fuel being generated as rapidly as it is consumed? Evidently not; otherwise there would have been slight ground indeed for the outcry which was so injudiciously raised about the prospective exhaustion of our coal-fields. We say "injudiciously raised" because the warnings were at once seized hold of by the coal interest, and employed as a weapon for striking an almost fatal blow at our manufactures, our commerce, our national wealth, and our domestic comfort. The waste of carbon can be checked only by the sacrifice of that profligate source of warmth the "cheerful fire,"—by the substitution, wherever possible, of water-power and tidal-power, and in brighter climates of direct solar-power, for steam-power obtained by the use of fuel,—and, where no such substitution is possible, by penalties upon a consumption of coal greater per horse-power than what can be proved practically necessary.

Phosphorus is an element which we are wasting in a different manner; not by oxidation, for it is most useful in combination with oxygen, but like the metals, by extreme comminution and promiscuous distribution. Here the total supply existing in Nature is small compared with that of carbon. Though phosphorus plays, most fortunately, a comparatively insignificant part in our industrial operations, yet in the chemistry of life it is second in importance to none of the elements. Without phosphorus none, at any rate, of the higher organisms, animal or vegetable, can possibly exist. All our crops require a due supply of it, and being the constituent of plant-food of which most soils are soonest exhausted it becomes, upon the principle laid down by Liebig, the measure of their fertility. Amongst the methods in which this important element is wasted, a prominent place belongs to the manufacture of lucifer matches. Three hundred thousand lbs. of phosphorus are made yearly in England and France alone, nearly the whole of which is absorbed in the match-manufacture. More than ten years ago Wagner calculated the total annual production for the

whole of Europe at about 540,000 lbs., requiring the consumption of about 6½ millions lbs. of bones, which would otherwise be available as manure. If we assume that the skeleton of an average man, when dry, weighs 20 lbs., this weight would furnish the bony framework for the bodies of 325,000 human beings. As each match is struck, its proportion of phosphorus is converted into phosphoric acid. Such conversion does not necessarily imply waste, since this is precisely the form in which phosphorus enters into the food of plants. But the phosphoric acid is scattered abroad in such minute portions that we may well question whether much of it will, in any reasonable time, find its way to its legitimate place, our fields and gardens. If we duly weigh how much nutritive matter is thus in effect destroyed, and how much potential life is prevented, we may well question whether the invention of "lucifers" has been quite as great a boon to mankind as is sometimes represented by platform-orators, and whether the power of instantaneously obtaining a light at all times and in all places is not being, to say the least, somewhat dearly purchased. It is perfectly possible to make matches without the consumption of any phosphorus at all—a fact too much lost sight of and too little acted upon, but which will ultimately force itself upon the public attention.

Bone-black or animal charcoal is largely used for water-filters, in sugar-refining, in the manufacture of blacking, and as a pigment, and contains about 70 per cent of phosphate of lime. That portion applied for filtering and for sugar-refining finds its way in course of time to the manure manufacturer, and ultimately to the soil. The amount used for blacking and for pigments is evidently squandered about in traces far too minute to be again collected by any human agency. To protest against so long-established and general a custom as giving a black surface to coverings for the feet is of course a hopeless task; yet if we consider that any material coloured black radiates heat more strongly than the same material coated with any other colour, we must admit the folly both of blacking, as applied to boots and shoes, and of the original blackening of shoe-leather before it leaves the hands of the currier. Our universal practice can only be considered rational if we wish to have our feet as hot as possible in summer and as cold as possible in winter.

Bone-ashes consist, of course, chiefly of the phosphate of lime, and are mainly utilised in agriculture. But a small portion is used in making cupells for the assayer, and in the manufacture of opal glass. The phosphorus that enters

into these compositions is doubtless lost,—certainly in the latter case,—but its quantity is not sufficiently large to render it worthy of especial attention. Of far more moment is the bone-ash wasted in refining argentiferous lead, no less than 17 lbs. being consumed for every ton of the metal operated upon. This is, if we refer to our former standard, a quantity nearly sufficient to build up the skeleton of a man. We are not aware that the bone-ash thus employed in refinery hearths is afterwards collected and purified from adherent traces of lead, so as to be fit for agricultural uses.

Many iron-ores contain phosphorus. Thus in vivianite, phosphoric acid may be present to the extent of 30 per cent. In a few instances this constituent is separated in an available form, but in the majority of cases it remains partly in the iron, to the great dissatisfaction of the manufacturer, who finds his product much impaired, and partly in the slags. Under both these circumstances it is generally wasted.

Phosphoric acid, combined with soda or potash, enters into the composition of certain "dung-substitutes" used by the calico-printer. Here, again, it must be regarded as ultimately wasted, since, when its purpose is served, it is washed into the sewers along with a variety of substances, many of them hostile to vegetable life, and therefore ill-fitted for application to the soil.

But even the phosphoric acid that is supplied to the fields in the forms of superphosphate, of bone-dust, Peruvian guano, farm-yard manure, night-soil, &c., and is there assimilated by the crops, is still afterwards in great danger of being turned aside from its normal channel of circulation, and thus of being substantially lost. Everyone in these days has learnt that animals, in their excreta,—solid, liquid, and aëriform,—give back all the matter which they have assimilated, and which has for a time formed part and parcel of their bodies. If the solid and liquid excreta, therefore, of all the animals fed upon the produce of a plot of land, of say 10 acres in size, together with all the waste or residual portions of the crops, are returned to that plot, its fertility—*i.e.*, its power of producing grain or vegetables, or of feeding cattle—will remain substantially unimpaired. But if this is neglected, or done only in part, a gradual decline of fertility must take place. We may, indeed, by importing bone-ash, Peruvian guano, and the like, keep up the needful supply of phosphoric acid in a 10-acre plot, or possibly in the whole of England; but this is, in familiar phrase, simply "robbing Peter to pay Paul." The whole

accessible store of phosphoric acid on the face of the globe, and consequently the total quantity of wheat and of beef that can be produced and the number of human beings that can be maintained, is lessened by every gallon of sewage we pour into a tidal river or into the sea. To put the fact in another light: every stroke of the costly and ornate pumping-engines at Barking Creek and at Crossness is destroying potential life, or rendering existence more difficult throughout the world! Surely this is an outcome of engineering skill on which the nineteenth century has small cause for self-congratulation.

It is indeed true that the phosphoric acid thus so bountifully poured into the sea may be, in the lapse of years, recovered in the shape of fish, serviceable for food or for manure; but this is a long, a tedious, and a doubtful circulation. If the excreta of men and animals are placed upon arable soil, their valuable constituents are rapidly made available and assimilated by plants, and may re-appear within a twelvemonth in the shape of food; but if poured into the sea their phosphoric acid, &c., will probably be first absorbed by low organisms, animal or vegetable. These will sooner or later become the food of fishes, and the matter in question may thus ultimately become available for human support.

In one case indeed we may, on the present system, receive back sewage matters much sooner than we wish. The particles of solids cast into the sea "hug the shore," just as corks floating in a basin of water keep to the side, and there are swallowed by those "scavengers of ocean," shrimps. Thus they may happen to be re-eaten by man even *before* assimilation. A chemist of the present day, who occasionally writes on sanitary topics from an original and independent point of view, remarks that thus—

——— "Even-handed justice
Commends the poisoned chalice to our lips."

A still more glaring example of waste occurs in the case of nitrogen, an element having important analogies with phosphorus, rivalling the latter in its relations to organic —and especially to animal—life, and playing a far more important and more varied part in the arts and manufactures. Manifold in appearance as are the methods in which this king of the elements is wasted, they may, with few exceptions, be reduced to one. To understand this great mode of waste we must glance at the two almost contradictory aspects which nitrogen is capable of assuming. On the one

hand, in its free gaseous state, as present in the atmosphere it is the very type of indifference and negation. We can merely say what it will not do. But on the other hand, when existing in its solidified combined condition, and especially in its organic compounds, it has more striking positive attributes than perhaps any other of the elements. No longer *azote*, as our French neighbours persist in calling it, it might rather be termed "zote,"—if such a word may be framed,—the very essence of life. It is an indispensable constituent of every vegetable seed and of every animal ovum. Without it blood, muscle, nervous tissue are impossibilities. But in addition to its functions in the processes of life, and therefore to its necessity in our diet, nitrogen—combined nitrogen, we must remember—has other properties which render it useful, or rather necessary, in numerous arts and manufactures. As a rule we shall find that if any organic compound, whether existing naturally or only produced by human intervention, possesses very striking attributes, such compound is nitrogenous. The most valuable fibres available for our clothing, the richest dyes procurable for their embellishment, the most precious medicines, the deadliest poisons, and last, though not least, those explosives so largely prescribed by modern humanitarianism in treating the diseases of bodies politic,—all these various bodies contain nitrogen as an essential constituent. Such manifold utilisation, in the present imperfect state of our knowledge, and in our still more imperfect disposition to make a rational application of what knowledge we possess, leads to a waste equally manifold. We say manifold in appearance, but still resolvable into one common principle—the conversion of solidified combined nitrogen into the free, inert, gaseous nitrogen which exists in the atmosphere. How vast are some of these forms of waste we will endeavour to show in some detail.

Let us, first and foremost, look at the very defective economy of nitrogen in the maintenance of human life, under existing conditions. In speaking of phosphorus we have already adverted to the truth so perseveringly and authoritatively enforced by Liebig,—that in growing crops upon land we take away from the soil certain constituents, and that its crop-producing power can only be prevented from diminishing by a systematic return of such constituents in the shape of the excretions of all living beings fed on the produce of the soil. Our neglect in fulfilling this condition is, perhaps, even more conspicuous in the case of nitrogen than of phosphorus itself. On an average the daily

excretions of every human being contain half an ounce of combined nitrogen. If we assume the population of the British islands at 32 millions, this amounts to a yearly quantity of 365 million lbs. This weight of combined nitrogen, if applied without loss to the soil and absorbed by food-plants, would, if calculated in the form of bread, be equivalent to 4380 million 4-lb. loaves! But how much of this fertilising matter is really returned to the land? The whole of the sewage of London—in other words, the whole of the excretions of its four million inhabitants—is substantially wasted by conveyance into the sea. Here alone we have a national loss of about 91 million lbs. of combined nitrogen. The same disastrous game, differing merely in unimportant matters, is played wherever sewage is being allowed to flow either directly into the sea or into a river or a canal, without any attempt to separate and retain its valuable constituents. Unfortunately the mischief is not confined to places where water-carriage has been adopted for the removal of excrementitious matter. If we look at the case of small country towns, of villages and of detached houses, where, theoretically speaking, the excreta are returned to the soil, we shall find that there is in practice a most sad deficiency. The dung-heaps and cesspools, in which the night-soil and urine are supposed to be stored up along with all other domestic refuse, betray their trust. On the one hand, the soluble matters, dissolved out by atmospheric waters, to which they are as a rule freely exposed, find their way into wells, ditches, ponds, and thus gradually into the next stream. We have often observed the completeness with which this form of waste is carried on in many villages in the Eastern counties. On one side of the road—or perhaps on both sides—is a ditch which receives from every cottage, every farmyard, as well as from manured fields, a tiny drain, rich in soluble organic matters. That these ditches are on amicable terms of "give and take" with the wells and pools which supply the village with its supposed potable water, is a by-evil with which we have at present no concern. On the other hand, the cesspools and manure-heaps are giving off their combined nitrogen to the atmosphere no less liberally than to the streams. Fermentation is going on unchecked; there is nothing which may serve to arrest and condense the volatile nitrogenous products, and when ultimately the contents of the heap or the pool are dug into the garden or the allotment-field, instead of containing, as

they should, the entire fertilising ingredients excreted by the inmates of the cottage, they consist chiefly of an impotent residuum. To call this a restoration to the soil of what has been taken from it is mere mockery. Not one-half the compounds withdrawn from our fields and gardens for the food of man, whether in town or country, are ever returned to where they belong.

So far, however, we have dealt merely with the waste of the fertilising matter in human excreta; but there is also the case of our domestic animals. When cattle and sheep are pasturing, and when horses are working in the fields, not only their dung, but their urine is deposited upon the soil. Under most other circumstances, however, the latter, which is the more valuable of the two, goes to swell the waste we have already mentioned. In the towns it is conveyed into the sewers, and in the country it escapes into ditches, adding thus no inconsiderable item to our yearly loss of combined nitrogen.

The remarks which we made on the waste of nitrogenous matters by the processes of putrefaction or fermentation going on in cesspools admit of a more extended application. Let us take the case of ordinary town-sewage. We can calculate with tolerable exactness how much nitrogen it ought to contain per gallon, if we know the gross number of the population and the total volume of the sewage. The figure thus obtained will, however, fall below the exact truth, since the proportion of nitrogen entering the sewers will be increased by the excretions of domestic animals and by the washings of slaughter-houses, &c. Yet if we take a gallon of the sewage, and submit it to actual analysis with the utmost care, we shall obtain a result very much short of the calculated percentage; some 20 to 50 per cent of the combined nitrogen, more or less according to circumstances, has escaped. In what form this loss takes place can scarcely be doubted. Were it given off as ammonia it could easily be detected; but ammonia does not readily evaporate from such dilute solutions, and at such low temperatures. We conclude, therefore, in accord with two chemists who have made the analysis of waters their speciality, and who on most points differ very widely in opinion, that the loss takes place in the form of free nitrogen. This we shall find to be the case whenever vegetable or animal matter containing nitrogen passes into a state of fermentation. A part of such nitrogen reappears as ammonia, and may be arrested by the aid of absorbents and of certain

acids and mineral salts; but a further portion is evolved as free nitrogen. As far as we are aware this loss becomes the greater the larger the quantity of organic matter which is allowed to collect.

Here, then, in the very outset of our enquiries, we have already met with a fearful waste of nitrogen committed in the normal course of daily life, and without taking industrial operations into account.

Let us next examine the manufacture of gunpowder, and its allies, gun-cotton and nitro-glycerin. The first of these substances contains on an average 75 per cent of saltpetre (nitrate of potash), equivalent to 10·2 per cent of combined or available nitrogen. Of this 10·2 per cent, 9·98 per cent —we might consequently say, practically speaking, the whole—escapes in the form of free nitrogen, and is consequently rendered useless. This waste will appear the more serious if we consider the enormous scale upon which gunpowder is now manufactured. It was calculated some years ago that our annual exports of this article alone amounted to 19 million lbs. weight. We cannot estimate the total quantity of gunpowder produced in the whole world at less than 100 million lbs. This represents 10 million lbs. of combined nitrogen withdrawn yearly from the world's available resources. Translating this into the shape of human food, the annual consumption of gunpowder means the destruction in advance of no less than 500 million lbs. of bread. We must remember, too, what this destruction means. If we were to take the ripe crops of wheat in some country to an extent capable of yielding 500 million lbs. of bread, and if—instead of allowing the grain to be gathered and put through the various processes which fit it for use—we were to have it ploughed into the soil, this conduct would be pronounced a criminal waste of the means of human subsistence; yet we should in reality have committed, comparatively speaking, little destruction. The buried nitrogenous matter would be still available for the growth of crops in succeeding seasons, and our supposed course of action, foolish and reprehensible as it would undoubtedly be, would delay rather than destroy the appearance of such combined nitrogen as food. But if we work up the combined nitrogen into gunpowder, and explode it, there is not merely delay, but what amounts to destruction—a destruction going on from year to year, and continually diminishing the amount of food which the earth is capable of yielding.

These considerations seem to us to supply a new argument against war, to which we would call the attention of such Peace Societies—if any there be—whose hatred of war is logically consistent, and is not merely assumed at times to suit the purposes of faction. Hitherto political economists and social reformers, especially of the Malthusian school, have looked upon war as one of the "positive checks" upon the increase of population. Whilst deploring the other evils brought on by international conflicts, they have regarded it as a set-off that every battle must decrease the proportion of eaters to the total amount of food. Had they, before advancing such opinions, taken counsel of the chemist, he would have told them that although their view might have been correct in the olden time, when men shot each other with the grey-goose shaft or cleft each other's skulls with the battle-axe, yet since

> "That villainous saltpetre hath been digged
> Out of the bowels of the harmless earth"

the case is totally altered. Our modern battles, how sanguinary soever, reduce the supply of food along with the demand. Every rifle and every cannon discharged *may* destroy actual life, but certainly *must* destroy the means of life. Each ounce of powder burnt means so much more nutriment withdrawn from our crops, so much less bread or beef producible, and so much less human life rendered possible in the future. Speculators on the population question must henceforth cease to regard war as one of their "positive checks." Indeed these same "positive checks" are becoming somewhat abridged. War, as we have seen, though waged upon a larger scale than ever, has been converted into an agency of an opposite nature. Pestilence is to be abolished by "sanitary reforms." What remains, then, but famine; and, unless we cease our systematic waste of the elements of food, that we shall certainly experience.

We shall, perhaps, understand more clearly the essential antagonism between food-raising and gunpowder-making if we consider the process by which saltpetre was formerly obtained in most countries of Europe. If we, in England, saw less of this operation than did most of our neighbours, it was because we imported the nitre from other parts of the Empire, where it was produced substantially on the same principle, even though without the conscious and intentional intervention of man. So-called nitre-beds were

formed and worked. To make these, earth was collected together from the sides and bottoms of cesspools, drains, urinals, and dunghills. It was mixed with plaster from old walls, and stratified with decaying animal and vegetable refuse. These beds or heaps were sheltered as far as possible from cold and rain, and were sometimes watered with urine. In course of time a whitish efflorescence began to appear in different parts of the heap. The earth was then dug up, lixiviated with hot water, and the solution thus obtained on concentration yielded crystals of saltpetre. Now it is evident that all the substances of which these nitre-beds were formed were in themselves nitrogenous, and capable of being used as manures. What was sold as saltpetre was simply so much robbed from the farm and the garden. As to the composition of the soil taken from the foundations of cesspools, stables, cow-houses, dunghills, &c., there can be now no doubt. Even the ground all about ordinary dwelling-houses was in former days saturated with excrementitious matter. We have adopted another form of waste; what our forefathers allowed to soak into the earth beneath and around their houses we run into the streams and seas. Our method may be a little more favourable to health, but it is waste all the same. The great fact that the saltpetre thus obtained from the nitre-beds was merely a transformation of the combined nitrogen put in them in various organic compounds was formerly not perceived. The nitre-bed was supposed, in some unexplained manner, to have the power of fascinating that chainless wanderer the free nitrogen of the atmosphere, and chaining him down as a slave to do man's work. Had, however, the constituents of the heap been accurately analysed,—a requirement utterly impossible when nitre-beds first came into use,—it would have been found that the nitrogen in the saltpetre obtained, instead of being more than equivalent to the nitrogen originally present, invariably fell short of it, and that a portion consequently of this element made its escape in the uncombined gaseous state. To this subject we shall be compelled to revert more fully below.

The Indian saltpetre, which till lately formed the bulk of our supply in England, was also obtained by the lixiviation of soils, which contained it as saline crusts. It was not, however, any and every soil which could be made use of, but such only as had become saturated with nitrogenous matter capable of yielding nitric acid by oxidation. The importation of saltpetre from India, therefore, was a robbery

of the soil of India, and lessened the crop-producing power of the world in general just as much as did the nitre-beds of Europe.

It is true that at the present day the great bulk of the saltpetre used, whether in the manufacture of gunpowder or in other arts, is obtained from a different source,—the deposits of nitrate of soda found on certain parts of the western coast of South America. From these vast deposits more than 4,000,000 cwts. are yearly exported to different parts of the world; yet even these beds are supposed to have been originally formed by the decomposition of nitrogenous organic matter, and though large they are by no means incapable of exhaustion. So long as we take combined nitrogen and set it at liberty then, no matter whence our supply is obtained, the ultimate result must be complete sterility.

The manufacture of gunpowder involves another kind of waste, which is more conspicuous in the present state of the trade than it was formerly. The nitrate of soda imported from South America, unlike the nitrate of potash, has the property of absorbing moisture from the air, and is hence utterly unfit for the preparation of gunpowder. Before it can be used for any such purpose it must be converted into common saltpetre by appropriate treatment with some salt of potash. Now, most unfortunately, potash—just like phosphorus and like combined nitrogen—is an important constituent of the food of plants, and we can only procure it by the old sin of robbing our crops. Formerly the salts of potash were obtained by burning timber to ashes and lixiviating the residue. Next, the salts left in the preparation of beet-root sugar were employed. In either case the substantial result was the same: unless we can show that pine-trees or beet-plants have the power of creating potash out of nothing, or out of some other elementary body, we must confess that they obtain it from the soil, and that the soil must ultimately become exhausted. But potash compounds are now obtained by mining, at Stassfurt and elsewhere? Granted, yet the supply—like that of nitrate of soda in Atacama—is not infinite.

The gunpowder manufacturers, or those who prepare their materials, compete with the farmer for the one and the other of these products, raising the price and hastening the day of exhaustion. As regards potash, however, the waste is of a less fatal character than the expenditure of combined nitrogen. Gunpowder, indeed, contains the

equivalent of 34 per cent of potash, and, according to our estimate of 100 million lbs. as the world's total annual production, about 34 million lbs. of this valuable body are thus withdrawn from the immediate service of agriculture. But the potash is not, like the nitrogen, withdrawn from utility. When a battle takes place all the potash contained in the gunpowder exploded gradually settles down upon the earth in the form of a fine dust, and, being washed into the soil by the next shower, becomes again available for the food of crops, and shares with the "red rain" the honour of making the harvest grow!

The other explosives in practical use—such as gun-cotton, nitro-glycerin in its manifold disguises and modifications, fulminate of mercury, &c.—all share with gunpowder the cardinal fault of unlocking combined nitrogen and returning it as an inert gas to the atmosphere. Wheresoever, therefore, man works by dint of explosions he is warring against life itself. The glorious victories of the warrior, the triumphs—often little less costly—of the civil engineer, the very "set pieces" of the pyrotechnist, the red fire in which the villain of the stage meets his retributive fate, and the squibs and crackers which have now for some two centuries been annually blazed away in honour of Guy Fawkes, are all bought at the cost of potential life, and all tend to make life more difficult.

We have now seen two of the great forms of the waste of combined nitrogen: wasted, as far as the excreta of man and animals are concerned, partly by conveyance into rivers and seas, partly by destructive fermentation, and in case of the explosives by a reduction differing from destructive fermentation merely in its more rapid character. But for both these kinds of waste there is at any rate the apology to be advanced that the nitrogen brought into play was essential. In other words, combined nitrogen is absolutely indispensable in the food of man and animals, and if their excreta are subsequently misapplied, and not returned to the soil, that does not condemn the original use of the nitrogenous matter. In the explosives, again, the nitrogen is not an inert constituent, but in the cases we have taken is essential. We cannot make either gunpowder, gun-cotton, nitro-glycerin, or picric acid without oxidised nitrogen, though we may and should most earnestly seek to produce explosives available for practical use which shall not require this expenditure of one of the main elements of life.

But there is another form of the waste of nitrogen more unjustifiable still. In pointing out some instances of this

kind of loss we must protest against the possible imputation of seeking to play into the hands of any sect of world-betterers and social reformers. We are merely judging certain practices which mankind adopted in days of greater ignorance than the present, according to the light of modern chemical science, and pronouncing our verdict without fear or favour, and utterly indifferent to the possible tendencies or applications of such decision.

It is a threadbare story to tell our readers that Nature does not present us with her treasures in a state ready for our immediate use. Sometimes they are mixed or combined with things useless, or even pernicious, as pyrites with arsenic, or coal with sulphur. More frequently the admixture may consist of an ingredient inferior indeed in value, but still capable of use. But if we, in endeavouring to turn to account some such plentiful and inferior article, waste a far more valuable product with which it is blended, we may confess ourselves guilty of egregious folly. Let us suppose an ore containing gold and copper in the relative proportions of a shilling's worth of the former to a pennyworth of the latter. Suppose, then, some metallurgist using this ore for the extraction of copper, and operating in such a manner as to render the gold utterly incapable of separation. He would be regarded as foolish and his process as wasteful, whether it was in itself remunerative or not. To waste a shilling's worth of gold for the sake of extracting a pennyworth of copper would be considered as a culpable abuse of natural resources. Now in the cereals we find a mixture of substances somewhat analogous to the case which we have, for illustration's sake, assumed. There is in them a very precious substance,—combined nitrogen in the form of gluten,—relatively small in quantity, but predominating in value; and there is a substance of a very much lower value, —a compound of carbon, hydrogen, and oxygen, in the form of starch. We are by no means denying that starch has its important uses, but it is a substance which Nature produces in almost indefinite amount, and its constituents are far more plentiful in the earth than is combined nitrogen. Hence we contend that if mankind, when in quest of starch or of some product of starch, take a substance containing starch in admixture with gluten, and waste the latter and more valuable product, they are acting just like the metallurgist whom we have been supposing, and are either ignorantly or knowingly squandering the resources of the world.

As the first instance of this economic sin we may mention the use of wheat-flour for purposes other than food. A very

serious quantity is consumed, without having undergone any previous chemical conversion, in the textile manufactures. It serves the sizer for communicating an artificial body to flimsy cotton goods, and is employed by the calico-printer in thickening his colours to the desired consistence. To give some idea of the extent of this waste we quote the following recipe from a recent work* :—

For medium sizing, take—

Flour, 3½ sacks	980 lbs.
Tallow...	180 ,,
Brown paraffin wax	5 ,,
White soap...	105 ,,
Soft soap	15 ,,
China clay	448 ,,

We have heard of 6½ sacks of flour mentioned as the daily consumption in a single manufacturing establishment.

From this we turn to the manufacture of starch. The demand for this substance in its ordinary commercial state is both varied and wide-spread. Its domestic use in stiffening linen, though most generally known, is probably not the most extensive application which it undergoes. Like flour, it serves for stiffening calico and for thickening colours, and is the raw material for the manufacture of so-called British gum. Of its functions in the sophistication of various articles of diet this is not the place to treat. Indeed none of the uses to which starch is put would in the least concern us were it obtainable, or at least were it ordinarily obtained, without the misappropriation of gluten, and the consequent waste of combined nitrogen. Unfortunately the chief materials selected, for the sake of convenience or on account of the quality of the product, by the starch manufacturer rank among the most important articles of human food, such as wheat, rice, maize, the potato, &c. If the whole of the nitrogenous matters could be separated out undamaged and in a state fit for human food, or even for the support of cattle, there would be little reason to complain; but no inconsiderable share of the gluten in the cereals undergoes fermentation, and much of its nitrogen is consequently wasted. Among the tasks which the industrial chemistry of the future will have to master are, therefore, some of the following :—

* Thomson's Sizing of Cotton Goods.

The obtaining a sufficient supply of starch from materials either non-nitrogenous or at least not adapted for human food, such as, *e.g.*, the horse-chestnut.

Or, if food-materials are still used in the starch manufacture, the complete utilisation of the nitrogenous compounds present.

Or, the complete replacement of starch, flour, &c., in textile manufactures, and in calico-printing by some inorganic substance.

Or, the supercession of "thickeners" by some improved method of applying colours—a step which some practical men consider as by no means out of the question.

From starch we pass to vinegar. This useful and extensively employed acid contains not a particle of nitrogenous matter, save in the form of impurities, not merely unessential, but damaging to its quality. All that is required for its manufacture is simply sugar, which is in the first place split up into carbonic acid and alcohol, which latter then undergoes a process of oxidation. If vegetable acids—such as the tartaric, citric, &c.—are present in the raw material, these not only effect an agreeable modification of the flavour, but contribute to the formation of traces of ethereal compounds, of most refreshing odour. This explains the excellence of the wine-vinegars of France, the apple-vinegars of America (which preserve the delightful aroma of ripe apples), and the vinegars occasionally met with in rural districts in England—made from cane-sugar, flavoured with the juices of fruits, or even flowers, such as the primrose or the cowslip. But the vinegar made for sale in England is unfortunately prepared from a nitrogenous matter, *i.e.*, malt, and we have thus combined nitrogen wasted in obtaining a product which can be procured in far superior quality from non-nitrogenous, or at least very sparingly nitrogenous, matter. True economy demands that bodies rich in combined nitrogen should be used for no purpose save where such nitrogen is essential, and should be restricted as far as possible to the production, directly or indirectly, of articles of food and medicine. The enunciation of this law brings us into open collision with widespread customs. We do not belong to those who pronounce alcohol altogether objectionable as an article of diet, and neither by moral suasion nor by legislative interference do we seek to abrogate its use; but as combined nitrogen is not required for the production of alcohol, we are compelled to include the use of nitrogenous products in the manufacture of alcoholic beverages among instances of that waste which we are explaining and

deprecating. The annual amount of malt made in this country is somewhere about 47 million bushels, or, in round numbers, 2000 million pounds. A very large part of the nitrogen originally present in the grain is wasted. A small quantity is indeed to be found in malt liquors as supplied to the consumer, but too little generally to be of appreciable dietetic value. The bulk is lost in the various stages of the process. Hence, from a chemico-economical point of view, the repeal of the malt tax, or any fiscal measure which shall encourage the application of grain or other highly nitrogenous matter to the manufacture of fermented or distilled liquors, would be a mistake. Well would it have been if the art of malting had never been invented, and if mankind had been content to procure their alcoholic beverages from fruits and from saccharine juices of comparatively low dietetic value, and have kept grain of all kinds for exclusive use as food.

But there is a use of nitrogenous matter even more palpably and lamentably wasteful than the manufactures of starch, of gum-substitutes, of vinegar, alcohol, &c., from grain. Indian corn is actually to some extent used as fuel,—a misappropriation rendered more feasible by the proportion of oily matter which it contains. That under such circumstances the bulk of its nitrogen will be wasted, by escaping in the free state, admits of no doubt.

Nitrogenous matter of animal origin is perhaps less systematically wasted than is the gluten of grain. Albumen obtained from eggs is indeed somewhat extensively applied by calico-printers as a mordant for inducing cotton to take up colours, especially the coal-tar dyes, in the same manner as do silk and wool. Unfortunately the albumen of blood, which is highly improper as food, has not yet been made available in all cases as a substitute for egg-albumen, inasmuch as it has not been practically procurable in a colourless state. Hence there is here room for a mordant capable of "animalising" vegetable tissues, and yet involving no waste of human food.

The use of wool in clothing does not involve the loss of combined nitrogen which might be at first suspected. The ultimate destination of the dust from the shoddy mill, and of the fibre itself when no longer capable of being re-spun, is to the manure manufactory and to the fields. With leather the case is less favourable. The raw hides of animals, though probably worthless as the food of man, are of great value as a nitrogenous manure; but after the tanning process they are scarcely, if at all, available as plant-food.

Leather-waste will lie for years in the soil undecomposed, and appears to exert no appreciable fertilising influence. Hence a non-nitrogenous substitute for leather would be a boon of no small magnitude to the world.

We have thus given a catalogue, by no means exhaustive, of the operations and processes, industrial and domestic, by which nitrogen is wasted. This waste, as we have seen, turns mainly on its transformation from the combined or solidified state to the free or gaseous condition as it is found in the atmosphere. But in this state, we shall be asked, is it not plentiful, almost to infinity, and does there not exist here one of those beautiful processes of circulation, of which we often read, by which it is restored to the combined or solidified state, and made again available? Yes, the store of atmospheric nitrogen is all but infinite, and we dare not assert that there is absolutely no natural process by which it can be re-combined with other elements; but these processes are slow in their operation, and, as far as we can judge, they cannot keep pace with our waste. They do not build up as quickly as we can destroy. We know that the atmosphere contains traces of ammonia, and that nitric acid in small quantity may be detected in rain, especially that which accompanies a thunderstorm; but we are still doubtful how much of this combined nitrogen has been really formed at the expense of the inert atmospheric nitrogen, and how much, on the other hand, is merely the decomposition of organic matter upon the earth's surface. It is ascertained that the electric spark, *i.e.*, lightning, on passing through a mixture of oxygen and nitrogen,—in other words, through common air,—effects the combination of these two elements so as to form nitric acid. Water holding in solution atmospheric air, as do all normal waters on the earth's surface, is found to yield nitric acid at the positive pole of the battery and ammonia at the negative. Hydrogen and nitrogen are also, according to the researches of Donkin, induced to combine by the action of the effluve, or silent electric discharge. But all such processes, however varied, have been found to be exceedingly slow in their action, and hence incapable of practical application in the arts. Ozone was once appealed to in every difficulty, but it has been proved incapable of oxidising free nitrogen to nitric acid in presence of water, as was suggested. Other reactions have been proposed, but none of them has proved distinctly and unequivocally successful as a means of combining the free nitrogen of the air into ammonia, or nitric acid, or cyanogen, or any other available compound. To solve this, the

king-problem of practical chemistry, is a duty still unfulfilled and a triumph still unearned. It may again be urged that processes which do not "pay" in our hands may yet prove successful in those of Nature, with whom time is no object. If ammonia is but sparingly manufactured in the upper regions of the atmosphere, it may perhaps be evolved by the aid of non-nitrogenous organic matter in some of the phases of fermentation or of eremacausis. Or the vital power of vegetables may solve the difficulty: the growing plant may arrest atmospheric nitrogen, and employ it in the formation of albumenoids. Method after method, thus pointed out as possible, has been carefully scrutinised. In most cases the experimental reply has been decidedly negative, in a few equivocal, in none distinctly affirmative. It is undoubted that the atmosphere superincumbent upon an acre of ground must supply combined nitrogen enough for all the vegetation naturally growing upon this acre, and must be able to keep up the supply from century to century. Were this not the case old Mother Earth would long ago have lost her green, flower-embroidered mantle. But to grow the crops needful for the support of our human population we are obliged to make, upon every single acre of arable land, demands a hundredfold greater than Nature ever makes upon an acre of forest, or prairie, or swamp, or moorland.

As we have thus increased our requisitions, and as we have still in another point deviated from Nature's plan by not restoring to the soil what we take from it, we meet the universally admitted fact that without a supply of nitrogenous manures the fertility of our fields declines. What proof other than this one fact is required to show that there is in the economy of the world no recuperative power equal to our present power of waste, and that we are thus rendering nitrogen unavailable more rapidly than it is being combined or made available? Our stock of solidified nitrogen, like our supply of solidified carbon, is decreasing, and must ultimately come to an end. The case of nitrogen is, of the two, by far the worse, because Nature is manufacturing fuel for us far more rapidly than she is producing albumenoids.

What then remains? The world may not, perhaps, be upon the whole more populous than it has ever been before; but thanks to some of the inventions upon which we so much pride ourselves, thanks to the industrial development which has characterised the last three centuries, and to which history records nothing similar, waste—destruction

of the matter most essential to life—has reached a height truly ominous and alarming to contemplate. It is sad to think that our civilisation, with all its glories, may be most aptly described in those lines from the "The Devil's Walk on Earth:"—

> "The pig swam well, but every stroke
> Was cutting his own throat."

We know now, approximately at least, the extent of the earth's resources. We have no more vast and scarcely trodden continents for us to discover. We can no longer calculate on finding coal, or phosphates, or other useful products, at any point where we may choose to dig. No more can we comfort ourselves with vague hopes that emptied mines and exhausted soils will spontaneously grow rich again, or that plants can create their own nourishment. Nor can we lay the flattering unction to our souls that for every utility no longer procurable a substitute will be found. The dreams indulged in by enthusiasts in the earlier portion of the present century—of a future measured perhaps by millions of years, in which mankind will be constantly improving in power, in knowledge, and in happiness—are being somewhat rudely broken. Science tells us, in unmistakable tones, that this earth cannot for ever afford a home to a race like ours. What, for instance, would be the condition of mankind were all the habitable parts of the globe as populous, as industrial, and as luxurious as are Western and Central Europe and the eastern part of North America? Whence would they all be able to import their needed supplies of food, of manures, and of raw materials for manufacturing purposes? Time was when England produced a sufficiency of food for her own inhabitants. Then came a time when she began to import, and that in ever-increasing proportions, both food and manures. Next we mark that the countries which formerly exported food and manurial matters—*i.e.*, ground bones, &c.—ceased to do so, became importers instead of exporters of both, and competed with us in the market. The Atlantic States of the American Union import manures, and can scarcely supply food for their home population. By-and-bye must come a time when Chili, California, South Russia, Hungary, will require all the wheat they can produce for their own consumption. The eyes of our political economists will perforce be opened to the truth that every country unable to feed its own population is in a dangerous predicament. The civilised world is now in the position of a spendthrift who is gradually

awaking to a faint consciousness that his resources are limited, and that "something must be done." Before us—and by "us" we mean not merely the British nation, but the entire civilised world—there lie open two courses. We may go on as we are now doing, increasing in wastefulness even more rapidly than in numbers, and presenting our drafts upon Nature till the reply at last comes "No effects." Or we may take the advice which Science has given us by the mouth of Liebig, and amend our ways. Pending the great discovery of the industrial utilisation of atmospheric nitrogen, we must look with a jealous eye upon every application of nitrogen, and also of phosphorus and of potash, other than for food or medicine. We must proscribe the use of nitrogenous bodies for all purposes to which such nitrogen is not essential. We must restore to the land all that we take from the land, in forms capable of assimilation. We must either seek to restrict the use of explosives, or we must find bodies of this class whose manufacture shall not rob our fields and gardens. We must obtain our sizings, our thickeners, our mordants, our vinegar, our alcohol, from bodies free from or at least poor in nitrogen. How much of the programme may lie within the bounds of possibility the future only can show. Shall we ever succeed in obtaining food direct from its inorganic elements without the tedious and circuitous interposition of plants and animals? If so, the future of the human race may be both longer and brighter than we can at present dare to hope. Meantime, to economise nitrogen, phosphorus, and potash, to recover these bodies from waste, and to find substitutes for their present "profligate" applications, is the most sacred task which the chemist can take in hand. The reforms which may shield us from occasional pestilence sink into insignificance compared with those required to guard posterity, in a not very remote future, from chronic scarcity, from recurrent famine, and from a wolfish struggle for food, in which man must relapse into a worse savagery than that from which he has emerged.

II. ON THE MOVEMENT OF MICROSCOPIC PARTICLES SUSPENDED IN LIQUIDS.

By Professor W. Stanley Jevons, LL.D., M.A., F.R.S.

If we see a thing moving of its own accord, this spontaneous motion is thought to be a sure sign that the object is an animal. Such an inference may be true of large bodies, but when we treat of microscopic objects the case is very different. Not only are minute vegetables, on the whole, as active as animals, but even mineral particles are found to be capable of moving as if endowed with life. This is a fact which has been familiar to microscopists for half a century at least. The phenomenon was made widely known by the celebrated botanist, Robert Brown, of the British Museum, who, in the course of some observations on the pollen of plants, discovered what he called *active molecules*, both in organic and inorganic bodies. He published his observations in two brief papers, which will be found in his "Miscellaneous Works," reprinted by the Ray Society (vol. i., p. 463). Considerable attention was drawn to the subject at the time, and the animated behaviour of particles became known as the *Brownian movement*.

Brown, however, as he himself points out, was not the first discoverer of the phenomenon. A few years previously John Bywater, a Liverpool man of some scientific merit, had published a tract (in 1819), re-issued in 1824 with additions, under the title "Physiological Fragments; to which are added Supplementary Observations to show that Vital and Chemical Energies are of the same Nature, and both derived from Solar Light." The greater part of this book is occupied with vague and worthless speculations, but his observations are better. He found that "small active linear bodies" can be obtained from matter in general. That they could not be really animated he inferred from the fact that they were yielded by coal just drawn from the pit, by the white ashes of coal, or by sandstone which had just been heated red-hot. Moreover, boiling kills animalcules, whereas he found that these lively particles could be boiled without suffering any loss of activity. Bywater came to the conclusion (p. 59) that these active bodies derive their vital

power from the water, and he acutely remarked that rocks generally undergo change when exposed to air and moisture.

Bywater, indeed, was far from being the first who examined the motion of minute particles; he was only the first who distinguished between the motions of living and dead particles. A series of physiologists—such as Buffon, Needham, Gleichen, Wrisberg, Müller, and Spallanzani—had been misled by these dancing particles into the belief that all bodies are formed of certain ultimate organic globules, which Spallanzani described as "animaletti d'ultimo ordine." Needham had, in 1749, published a quarto tract called "Observations upon the Generation of Animal Substances," in which he described the motions of little *eels* and other small bodies. Buffon, who was acquainted with Needham, seems to have been thus led to start his celebrated theory of *organical parts*. He says*—"God . . . has not only given form to the dust of the earth, but he has rendered it living and animated by enclosing in each individual a greater or less quantity of active principles, of organic living molecules, indestructible and common to all organised beings."

We may go back still further; for in the "Philosophical Transactions" for 1696 (vol. xix., p. 280) is to be found a remarkable account of some microscopical observations by Stephen Gray, made by microscopes, or rather lenses, of very primitive construction; yet Gray seems to have succeeded in viewing these animated particles, and, considered by the light of later observations, the following passage is well worth quoting:—"I have examined many transparent fluids, as water, wine, brandy, vinegar, beer, spittle, urine, &c., and do not remember to have found any of these without more or less of the bodies of these insects; but I have not seen any in motion except in common water, that has stood for sometimes a longer, at others a shorter time, as has been observed by M. Leeuwenhoek; though I do not remember he has observed that they are existent in the water before they revive. In the river, after the water has been thickened by rain, there are such infinite numbers of them that the water seems in great part to owe its opacity and whiteness to these globules. Rain-water, so soon as it falls, has many, and snow-water has more, of these globules." It is probable that these moving globules were not really insects, but inorganic particles moving in the way we have

* Histoire Naturelle, redigé par Sonnini, tome xxiii., p. 2. See also tome xvii., ch. 6, p. 231.

to investigate. Leeuwenhoek was the most celebrated of the early microscopists, and appears to have frequently observed the motions of small bodies.

One astonishing fact about this subject is the small attention paid to it in recent years. Microscopists have continually had the phenomenon under their eyes, and it has often been mentioned as a mysterious one which might lead to great discoveries. Faraday's attention was drawn to the matter by Brown's tracts, and he gave a Friday evening discourse on the "active molecules." Part of the notes of this lecture have been published by Dr. Bence Jones, in his "Life of Faraday" (vol. i., p. 403) and are very interesting; they end thus:—"*Lastly, the relation of these appearances to known or unknown causes.* Analogy to other moving particles. Camphor. Supposed facility of explanation, *not camphor motion.* Takes place within pollen. Under water, inclosed by mica or oil—not *crystalline* particles—not attraction or repulsion. Does not consist in receding and approaching—*not evaporation* answered as before—not currrents, too minute—oscillation—consider current in a drop—when currents, motion very different—not electricity of ordinary kind—because do not come to rest, seen after hours—so that the cause is at present undetermined.

"Mr. Brown supposed to be careless and bold, is used to microscopical investigations—has not yet been corrected—assisted by Dr. Wollaston—so that carelessness can hardly be charged. Then, what does Mr. Brown say? Simply that he cannot account for the motions.

"Many think Mr. Brown has said things which he has not—but that is because subject connects itself so readily with general molecular philosophy that all *think* he must have meant this or that—as to molecules, by no means understand ultimate atoms—as to size, says that solid matter has a tendency to divide into particles about that size—pulverisation and precipitation—if smaller, which may be, *are very difficult to see*—does not say that all particles are alike in their nature, but simply that organic and inorganic particles having motion—motion cannot be considered as distinctive of vitality—connection with atomic or molecular philosophy."

It is plain that Faraday, although he correctly puts aside many erroneous suggestions, could not explain the phenomenon; but it is strange that in his long life of indefatigable research he never recurred to the subject, nor did any other eminent physicist undertake the task. A few incidental remarks upon the subject are to be found in books on the

microscope, as in that of Dr. Carpenter (4th edition, p. 169), or in Griffith's "Micrographic Dictionary," article "Molecular Motion" (3rd edition, vol. i., p. 498).

The French naturalist and microscopist, Dujardin, appears to have paid special attention to this Brownian movement, which he describes with more minuteness and accuracy than any previous observer.*

Griffith, the editor of the "Micrographic Dictionary," seems to be almost the only other observer who has specially investigated this curious subject. His results are mostly correct as far as they go.† He finds that all kinds of ponderable matter, whether organic or inorganic, exhibit these movements when reduced to a fine state of division, and suspended in a liquid not too viscid or tenacious. He satisfied himself that a single particle moves independently of surrounding particles. He rejects the explanations previously suggested, such as the influence of evaporation, and, after unsuccessfully trying some experiments with electricity, leaves off with the candid conclusion that he knows no cause for the phenomenon.

For a number of years past‡ I have occasionally occupied leisure time with experiments on this subject. For the microscopic observations a good compound instrument is desirable, if not indispensable. I have used an excellent instrument by Dancer, of Manchester, the quarter-inch objective of which, with the lowest eye-piece, usually suffices; but, to observe the movements in all their perfection, I have found a good one-eighth inch objective, with a C or D eye-piece, to be useful.

One difficulty is to find a convenient name for the phenomenon. Brown called it *molecular movement*, and it is often so called in microscopic books; but this is a very bad name, as the particles moving are of course not molecules. Some writers have called it the *Brownian movement;* but this two-worded expression is very inconvenient, is not in any way descriptive, and gives perhaps undeserved honour to Brown, who was not its first discoverer, and did little more than make it generally known. Dujardin described the motion as one of *titubation*, from the Latin *titubatio*, which means staggering or wavering; but this word is not elegant, and

* Nouveau Manuel complet de l'Observateur au Microscope. Manuels Roret, Paris, 1843, pp. 58 to 60.

† London Medical Gazette, 1843, vol. ii., pp. 502 to 506.

‡ A brief preliminary account of my experiments was given in the Proceedings of the Manchester Literary and Philosophical Society for January 25th, 1870 (vol. ix., p. 78).

has not since been used. I have therefore ventured to coin, or rather adopt, a new word, and call the motion *pedesis*, from the Greek πηδησις, *leaping* or *bounding*, this being the correct description of the phenomenon when seen in perfection. We also have the advantage of the perfectly classical adjective *pedetic*, from the Greek πηδητικος.

The pedetic movement cannot be better seen than by taking a drop of old common ink which has been exposed to the air for some weeks, and examining it under thin glass with a magnifying power of 500 or 1000 diameters. An infinite multitude of minute black particles will be seen, all in such rapid motion as to cause a boiling or swarming appearance. This is, in fact, a wonderful sight. In new clear ink the particles are fewer and very minute, but all in motion, as far as can be seen. Almost any kind of fine clay mixed with pure water shows pedesis, and muddy rain water from a macadamised road, if properly examined, will soon convince the observer what a common phenomenon it must be. Perhaps the best possible exhibition of the motion is to be got by grinding up a particle of pumice-stone in an agate mortar, and mixing it with distilled water. The minute angular particles will be seen under the microscope to leap and swarm about with an incessant quivering movement, so rapid that it is impossible to follow the course of a particle, which probably changes its direction of motion fifteen or twenty times in a second.

It is very difficult to measure or describe the motions with accuracy, and they vary much according to circumstances. The distance through which a particle moves at any one bound is usually less than 1-5000th part of an inch. The motions much depend upon the sizes of the particles. Those of a greater diameter than 1-5000th of an inch are seldom seen to move, and the motion is more marked as the particles are smaller. Exceedingly minute particles may sometimes be seen literally to skip and dance about, as in the case of some minute particles of metallic antimony which Mr. Dancer,* the microscopist, of Manchester, showed me with an excellent 1-8th inch objective.

There is no apparent lower limit to the size of moving particles, and down to the 1-50,000th or 1-70,000th part of an inch the movement certainly becomes more and more remarkable.

* Mr. Dancer, F.R.A.S., studied this subject at intervals for thirty years, but I do not know that he has published any results except those given in the Proceedings of the Manchester Literary and Philosophical Society for January 25th, 1870 (vol. ix., p. 82). Mr. Dancer found that diamond-dust and graphite could be made to show the motion.

The substance which I have found to be most convenient for experiments on pedesis is fine pure *china clay*, or *kaolin*. This clay is used in photography, and is to be purchased very carefully washed and prepared for the purpose. A small quantity of the clay shaken up with pure water makes a milky liquid, a drop of which being placed under a piece of thin glass will show the motion in great perfection; but finely-powdered glass, earthenware, and in fact almost any very finely-powdered substance, will do nearly as well. Among vegetable substances gamboge may be mentioned as readily giving minute particles, which quiver and dance about in a wonderful way.

In endeavouring to discover the cause of this phenomenon I made a certain number of experiments to test the validity of various explanations which had been offered. It has often been suggested that the motion is excited by rays of light or heat falling upon the liquid, and Crookes's radiometer now shows that in some cases it is possible to convert radiant energy directly into the energy of visible motion. But this idea was easily and completely disproved. Taking several active substances, such as kaolin, road-dust, red oxide of iron, and mixing them with distilled water, I examined them in the microscope, causing the direct rays of the sun, somewhat concentrated by a lens, to pass straight through the glass slide into the microscope. I then interposed a dark glass alternately between the sun and the specimen, and between the eyepiece and the eye. While the amount of light reaching the eye remained nearly the same, the intensity of the rays falling on the moving particles was probably several hundred times as great in one case as the other; yet the vibrations of the particles proceeded in exactly the same manner in comparative darkness and in intense light. Different coloured glass screens—purple, yellow, rose-coloured, &c.—were also tried, but no difference in the motions was apparent. The same conclusion was indirectly obtained, after the connection of pedesis with the supension of particles in water had been ascertained, by taking two tubes of china clay and pure water, putting one in a dark place, and exposing the other to the direct rays of the sun for three hours. No difference in the rapidity of subsidence was apparent.

It is asserted in Dr. Carpenter's work on the microscope (§ 130) that this so-called molecular movement is greatly accelerated and rendered more energetic by heat. "This," he says, "seems to show that it is due either directly to some calorical changes continually taking place in the fluid,

or to some obscure chemical action between the solid particles and the fluid, which is indirectly promoted by heat." Although I believe there is obscure chemical action, my experiments lead me to think that the effect of heat is rather the other way, and that increase of temperature decreases the motion. I was not able, indeed, to perceive any difference produced by warming the microscope plate; but as this is a difficult and uncertain experiment, I tried the matter indirectly. A mixture of charcoal-powder and boiled water was surrounded with ice, and a similar mixture was placed in boiling water and maintained at 100° C. At the end of one hour the heated mixture had deposited nearly all the charcoal, whereas the ice-cold water still had as much in suspension after eight hours. A similar experiment with china clay gave like results. This is the more surprising and conclusive inasmuch as the heated mixture would be the most likely to be disturbed by convection currents. It is well known that heat causes the aggregation of many precipitates, and I suspect that this will in most cases be due to the diminution of pedesis, and I shall point out that this is probably explained by the increase of electrical conductivity of liquids produced by rise of temperature.

It may still be urged that a liquid while receiving or parting with heat might show increased pedesis. To test this I took a tube containing china clay and water, and during two days frequently suspended it before the fire, allowing it to cool in the intervals. An exactly similar tube was sunk in sawdust which had been lying for several years undisturbed in a wine-cellar. After remaining fifty-two hours in nearly perfect darkness and equilibrium of temperature (about 9° C.), the latter tube was found to contain more clay in suspension than the one which had been moved about and warmed many times. Even after seven days the buried tube still showed a slight cloudiness. This was a very striking experiment, and quite convinced me that no extrinsic causes are concerned in the matter.

As it might be thought that the movement is connected in some way with the shape of the particles, I compared in the microscope the fine needle-shaped particles of asbestos dust with the spherical globules of milk, the minute spheres of gamboge, the flat particles of talç, the small cubes of galena, and the wholly irregular fragments of glass. As particles of all these various forms show pedesis, no particular form can be essential to its production; but it seems likely that, *cæteris paribus*, sharp-pointed and irregular particles oscillate most intensely and rapidly. Spherical

particles exhibit rather a gentle swaying backwards and forwards in no determinate direction: this motion can be well studied in common milk, or in a mixture of oil and water thoroughly rubbed together.

It is natural to enquire how long the pedetic movement will go on. Does it exhaust itself rapidly? The experiments which I have made lead to the opposite conclusion in a wonderful degree. Ink which is many months—and possibly years—old, and which has been long exposed to the air, exhibits the motion in perfection, as already stated. Again, by means of a pipette, I took a little of the lees from a bottle of port wine which had been lying undisturbed in a wine-cellar for several years, and placed a drop under the thin glass cover of the microscope plate, with the least possible exposure to air. A slow distinct motion of the particles was apparent, and it was not increased when some of the dregs had been shaken up in a bottle with air. This experiment leaves no doubt in my mind that the sediment of port wine is in a state of perpetual motion, until it finally settles down and attaches itself to the glass. But the most surprising fact elicited in the whole enquiry was furnished by two glass tubes containing china clay and distilled water, which I corked up in November, 1869, and laid in a drawer usually opened several times in the day, so that they would be shaken up every now and then; frequently, too, they were shaken up by hand. At long intervals the tubes were opened, and drops of the milky liquid examined. Comparing the motion of the particles with that of newly-mixed china clay and water, *no diminution of motion was apparent;* on the contrary, *the motion seemed to be even more remarkable than in a fresh mixture.* This observation was confirmed by the suspensory power of the liquid. On several occasions, in 1876 and 1877, I have shaken up the tubes and placed them upright in a spot shielded from light. After a lapse of eleven days I have found a slight cloud of clay still in suspension in the lower part of the water. Thus, after a trial of eight or nine years' duration, we meet with the astonishing fact that the suspensory power and the pedetic motion apparently increase with time. In fact this pedetic motion seems to be the best approach yet discovered to a perpetual motion. The above observations are quite in agreement with the statement of Dr. Carpenter ("Microscope," § 130, p. 169), that the movement has been known to continue for many years in a small quantity of fluid enclosed between two glasses in an air-tight case. Faraday, as we have seen, discarded ordinary electricity as

the cause of motion, because the particles do not come to rest after hours. Nevertheless I believe that motions lasting for several years are not inconsistent with an electrical explanation.

Prof. Tyndall has quite recently expressed his opinion that this motion of particles is due to "surface-tension," but he cannot be aware of the length of time during which the motion will continue. It is obvious that from surface-tension we cannot derive a long-continued supply of energy, as is required to explain the phenomena.

One of the most important questions to decide is naturally what substances exhibit these movements, or do all substances exhibit them? I have tried many experiments with a view of answering these questions, and though at times there seemed to be particles of the proper size which did not move in pure water, subsequent trials often threw a doubt upon the conclusion. I hesitate, therefore, to affirm that there is any substance which may not be reduced to such small particles, and so suspended in water as to show pedesis; but there are certainly great differences in activity. Silica, in all its insoluble forms and compounds, is especially remarkable for the activity of movement. Clay of several kinds, earthenware, fire-brick, common brick, clay slate, silicified wood, glass of several kinds, talc, mica, pumice-stone, asbestos, basalt, granite, slag from iron and other furnaces,—also many silicious minerals, such as topaz, tripoli powder, glacier sand,—all exhibit the movement in a high degree of intensity; and these instances are probably sufficient to show that all silicious compounds, and accordingly the greater part of known minerals and rocks, yield very active particles. But the state of composition is not requisite to the effect; for the purest quartz crystal, agate, cornelian, chalcedony, or the finest white sand, when reduced to dust, show almost equally active motion. On the whole the silicates and silica exhibit pedesis in the highest perfection.

The substances, on the other hand, which seem to be least remarkable for pedesis, are oxides and other compounds devoid of silica. I may mention especially chalk, fluor spar, hematite iron ore, galena, oxide of tin, barium sulphate, calcium sulphate, titaniferous sand, bone-dust. Various other substances—such as charcoal of several kinds, coal, coke, emery powder, amorphous phosphorus—show the motion readily. The metals iron, steel, gold, and platinum were tried at first without success, probably because it was not easy to reduce them to sufficiently minute particles;

but the brittle metals, antimony, arsenic, and bismuth, were readily ground into fine powder, and showed active motion. Afterwards I obtained much motion with steel, lead, bronze, and standard silver and gold coin, the particles being produced by rubbing the metals on a smooth hone. Sulphur could not be ground up fine enough; but some sulphuretted hydrogen water having become decomposed, the precipitate of sulphur was found to consist of very minute particles, probably about 1-30,000th of an inch in diameter, in a state of extraordinarily active motion. It is certain that substances of the most widely different chemical characters will exhibit the phenomenon, and it is difficult to establish any clear differences in the activity of the motion as connected with the chemical nature.

As the form and chemical nature of the particles and the physical circumstances of the liquid seemed to throw no light on the cause of pedesis, it remained to consider the chemical character of the liquid. Striking differences in this respect were soon detected. If, instead of mixing china clay with pure water, we mix it with a very dilute solution of sulphuric acid, say one part in a thousand, an extraordinary difference results. Under the microscope the particles of clay are seen to aggregate together, and rapidly sink down upon the glass slide. The pedetic movement is almost entirely destroyed, or, if any particles move at all, it is only exceedingly minute ones. The same effect is produced by almost any mineral acid, and, as a general rule, by all salts and other soluble substances, with certain significant exceptions afterwards to be described. Before describing the experiments in this direction, however, it is necessary to point out the intimate connection which exists between pedesis and the suspension of particles in liquid.

It seems surprising that the conspicuous phenomena arising from the suspension or precipitation of solid particles in liquids have not been more carefully studied. Besides being full of scientific interest, they have great practical importance in questions of water supply and sewage treatment. A few experimenters have touched upon the subject. The microscopist Dujardin, already quoted, hits the right nail on the head when he says (p. 60)—"Ce mouvement Brownien joue un rôle important dans certains phénomènes physiques; c'est lui qui empêche les eaux troubles de se clarifier promptement par le repos."

A few other casual remarks to the same effect may be quoted from several sources, but I am not yet aware of any connected series of experiments upon the suspension of

powders prior to those which I made in 1869, the results of which were briefly published in the "Proceedings of the Manchester Literary and Philosophical Society" for January 25th, 1870 (vol. ix., p. 78). Since then several experimenters have treated the subject. Mr. Wm. Durham, F.R.S.E.,* has correctly apprehended that the suspension depends upon the non-conducting nature of the fluid; but he is evidently wrong in supposing that the electricity is generated by the falling of the particles through the fluid, inasmuch as the pedetic movement, which favours suspension, is greatest when the falling of the particles is slowest. Mr. David Robertson has tested the precipitating power of salt, or even slightly brackish water,† and Mr. Wm. Ramsay, principal assistant in the Glasgow University Laboratory, has attempted an explanation of the fact with which I cannot agree.‡ He attributes the varying rapidity of the settling of clay suspended in saline solutions to the varying absorption of heat by the solutions, whatever this may mean.

In the "Chemical News" for August 31, 1874 (vol. xxx., p. 97) it was pointed out that Dr. Sterry Hunt read a paper before the Boston Society of Natural History, on February 18th, 1874, in which he alluded to the rate of settlement of clay in the water of the Mississippi River. The suspended matter, chiefly clay, amounts to about 1 part in 2000 parts of the water, and takes from ten to fourteen days to subside. But he observed that additions of sea-water, salt of magnesium sulphate, alum, or sulphuric acid, rendered the turbid water clear in twelve or eighteen hours. He thus explained the ready precipitation of the suspended clay when the river-water comes into contact with the salt water of the Gulf of Mexico, causing great deposits of fine mud, and helping us to understand the origin of the accumulation of clay slates in various geological formations. My experiments in 1869, and since, entirely confirm these observations of Dr. Hunt, and lead me to think that the pedetic movement must have played a most important part in geological processes.

The power of salts to precipitate suspended particles has long been known practically. In some parts of the world

* Abstract of a Paper read before the Royal Physical Society of Edinburgh, January 28th, 1874 (Chemical News, August 7th, 1874, vol. xxx., p. 57).

† "Note on the Precipitation of Clay in Fresh and Salt Water."—Transactions of the Geological Society of Glasgow, vol. iv., p. 257. See Whitaker's excellent Geological Record for 1874, p. 182. I have not been able to refer to the original paper.

‡ Abstract of the Proceedings of the Geological Society of London, No. 315, March 8th, 1876, p. 1.

alum has been dissolved in river-water to clear it. I presume that we may in the same way explain the use of the *Strychnos potatorum*, or clearing nut, the seeds of which are used in the East Indies for the purpose of clearing muddy water. One of the seeds is well rubbed for a minute or two round the inside of the vessel containing the water, which is then left to settle; in a short time the impurities fall to the bottom. Bitter almonds are employed for the same purpose in Egypt, and those of the Kola or Sterculia in Sierra Leone. These methods are probably due to the destruction of pedesis.

The connection between pedesis and suspension is simply this;—In the absence of pedesis suspended particles attract each other and become aggregated together into little groups, which then acquire sufficient weight to force their way down through the resisting liquid. This effect I have observed some hundreds of times, both in and out of the microscope. If a little china clay be mixed up with water containing 1-10000th or 1-10,000th part of sulphuric acid, the milky liquid will presently assume a flocky curdled appearance, and after a time the little flocks will subside, streams of clear water making their way up between the interstices. The rate of subsidence is doubtless affected by other circumstances, such as the specific gravity of the liquid, the smallness of the separate particles, and their shape; but it is the aggregation of particles into groups which mainly determines precipitation or suspension. As a rain-drop is to a cloud particle, so is a group of clay particles to the minute suspended particles. Prof. Stokes has shown that the resistance experienced by the minute particle in falling through a fluid is comparatively enormous, so that there is really no further mystery in the falling of curdled precipitates. But pedetic motion prevents the formation of groups; it keeps each minute particle—say 1-1000th of an inch—from each other particle, so that each encounters the separate resistance of the fluid.

Dujardin, indeed, thought that the pedetic motion caused particles to diffuse themselves through water. "C'est lui aussi qui répand dans toute une masse d'eau, et y tient en suspension les particules désagrégés d'un corps animal ou végétal qui se décompose." In this he is probably wrong, for if a layer of clear distilled water be placed above a mixture of china clay and distilled water no diffusion of the clay upwards will be detected. It is difficult to make the experiment in an accurate manner; but if we consider that the pedetic motion, as seen in the microscope, is alternating

and vibratory, and quite indeterminate in direction, and that any one jump of a particle probably never exceeds 1-10000th part of an inch at the most, it becomes exceedingly unlikely that any particular particle should make ten such steps in the same direction. According to the theory of probability the proportion of particles which would succeed in moving any perceptible distance would be imperceptibly small.

Proceeding with experiments upon many solutions and liquids, this fact came out by degrees more and more clearly—that it is pure water which exhibits pedesis in the highest perfection. So remarkably is this the case that *the suspension of china clay may be used as a test of the purity of water.* Even the air and carbonic acid, which is usually dissolved in water, produces a perceptible difference. If a milky mixture of china clay and distilled water be divided into two portions—and while one is well-boiled in a glass tube and then corked up, the other is shaken in a large bottle with air, and then left to stand in a similar glass tube—the clay will be found to remain longer in suspension in the boiled water. In a similar way, if china clay be mixed with various specimens of water,—such as spring water, river-water, rain-water, boiled rain-water,—it is possible to detect differences in the rate of precipitation. In experimenting upon such mixtures it will be noticed that the topmost layer of water, to the depth of one-eighth or one-quarter of an inch, becomes very clear before the rest of the liquid: this may probably be attributed to the air dissolved by the water with which it is in contact. The quantity of air absorbed by water is probably quite competent to produce this effect, as it often amounts to several hundred-thousandths by weight of the water.

It has been stated that, as a general rule, all substances dissolved in water tend to prevent pedesis; but it is easy to detect differences in this effect. The mineral acids—sulphuric, nitric, or hydrochloric—have an extraordinary effect, and it is possible to detect one part of sulphuric acid in a million parts of water by the precipitation of china clay. Caustic alkalies and metallic salts have a less, but still a great, influence. I have tried the nitrates of copper, mercury, and silver, the sulphates of copper and zinc, chloride of gold, acetate of lead, bichromate of potash. Various other acids—such as oxalic, fluoric, hydro-fluosilicic, citric, arsenic acids,—also have considerable precipitating power. Somewhat lower in the scale may be placed such salts as carbonate of potash and soda, chlorate of potash. Among

solutions of still less power may be mentioned common salt, sulphate of potash, iodide of potassium, nitrate of potash, cyanide of potassium, ferrocyanide of potassium, lime-water, &c. My recorded observations amount to nearly eight hundred, and the solutions named were tried not only in different strengths, varying according to circumstances, from one part in ten to one part in a million, but they were tried with various suspended powders, such as charcoal, red oxide of iron, amorphous phosphorus, precipitated carbonate of lime, red oxide of lead, black oxide of manganese, and occasionally with other substances. I do not think, then, that I can be much mistaken in my chief conclusions.

While salts and substances dissolved in water prevent pedesis, as a general rule, there are certain remarkable exceptions of great significance. One of these is pure caustic ammonia, the liquor ammoniæ of the apothecaries. One per cent of ammonia will be found to have little or no effect in precipitating powders from water. The compounds of ammonia are quite different from ammonia itself, and, so far as I have observed, all have precipitating power, though apparently less than that of most other salts. Other very notable exceptions are boracic acid and silicate of soda. Upon these, especially the latter, I have tried many experiments. With 1 per cent solutions of these substances clay was observed to remain in suspension for twenty-four hours, and in some experiments a slight cloud of clay remained after five days. Indeed I believe that silicate of soda has a positive retarding power, and prolongs the suspension of fine particles. This salt will also neutralise the precipitating power of the acids: when clay is mixed, for instance, with water containing one per cent of silicate of soda and one-tenth per cent of nitric acid, the acid combines with the soda, liberating silicic acid. Now soluble silicic acid has no perceptible power in preventing pedesis, as proved by experiments upon two samples of very pure silicic acid, carefully prepared for me by Prof. Schorlemmer, of Owens College, for which I am much indebted to him. Clay, mixed even with a *concentrated* solution of *pure* silicic acid, will show pedetic motion of minute particles in the microscope.

One of the most extraordinary facts connected with this subject is the power which gum arabic possesses of increasing the pedetic motion. The more inactive substances, such as powdered galena, oxide of iron, or carbonate of lime, when examined with a weak pure solution of gum arabic (say 5 per cent), exhibit much motion, and there is the corresponding absence of precipitating power, or rather a power

of maintaining substances in suspension. It is obviously on this account that gum arabic is an invariable component of common ink, though the quantity specified in various recipes ranges from about 1 to 5 per cent. The extraordinary pedesis seen in old ink is due to the gum arabic, and perhaps also that seen in gamboge.

It will not be difficult now to arrive at an explanation of the pedetic motion. When we compare the substances which do not prevent the motion with those which do, it becomes apparent that, with some doubtful exceptions, they differ widely in the power of making water a conductor of electricity. The following is a quotation from one of Faraday's earlier researches:*—"Some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the (conducting) power of water enormously, whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do not produce a change sensible to the voltameter. Ammonia produces no effect, but its carbonate does. Sulphate of soda, nitric, and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar; the test being a voltameter."

The argument in the case of pedesis is exactly analogous to that which Faraday employed in his inquiry into the production of electricity by Sir W. Armstrong's electrical boiler. Faraday found that the machine must be supplied with pure distilled water in order to yield much electricity. The smallest drop of sulphuric acid, or a little crystal of sulphate of soda, dissolved in the water, prevented the evolution of electricity. "So also did the addition of *any* of these saline or other substances which give conducting power to water." Then Faraday argued in this way:—"As ammonia increases the conducting power of water only in a small degree, I concluded that it would not take away the power of excitement. Accordingly, on introducing some to the pure water in the globe, electricity was still evolved. But the addition of a very small portion of dilute sulphuric acid, by forming sulphate of ammonia, took away all power."† Even common town's water was found by Faraday to be unsuitable to the production of electricity. The analogy of these circumstances to those of pedesis is so remarkable that little doubt can be entertained that the same explanation applies. *It is perfectly pure water which produces electricity and pedesis.*

* Experimental Researches in Electricity, vol. i., p. 432, § 1355; see also p. 161, § 554.

† *Ibid.*, vol. ii., pp. 110, 111, § 2090 to 2094.

Almost all soluble substances prevent both one and the other phenomenon; but ammonia is one of a few exceptions—it allows both of electric excitation and pedesis. Boracic acid is another exception, and gum a third one. There are difficulties, no doubt. Silicate of soda and silicic acid were not tested by Faraday. Tartaric, citric, and acetic acids are mentioned by him as giving little conducting power to water, but they arrest pedesis in a considerable degree. But it is to be remembered that the conducting power of liquids and the connected effect of electrolysis are very imperfectly understood, and few researches on the subject have been undertaken since these old ones of Faraday.

The proper way of settling the question, no doubt, is to make direct experiments upon silicic acid and the other substances which allow or promote pedesis. This I attempted to do, immersing two platinum points, about 1-60th of an inch apart, in various solutions, and connecting them with two of Grove's cells and an ordinary galvanometer. The results did not altogether agree with the theory, but there are many doubtful points about the matter. It does not follow that the conducting power of a liquid through 1-60th of an inch, with the power of two of Bunsen's cells, will correspond to the conducting power through 1-5000th of an inch at a very low tension; and there are other difficulties in the matter. In spite of some discrepancies and failures, I still think the analogy between pedesis and Armstrong's electrical machine so strong as to leave little doubt that pedesis is an electrical phenomenon. Various reasons may be given for regarding this conclusion as probable.

There is no doubt that pure water may produce electric tension. If pure water be put into an ordinary electric battery, the tension produced is supposed to be the same as with dilute sulphuric acid. In fact the acid is only added to make the water a conductor, without which the current is imperceptible. Then, again, there is no doubt that, when water is in contact with silicates, some chemical action goes on, for silicic acid is invariably found in all water which has been in contact with the earth. It is perfectly well known that all clays, and also all rocks exposed to air and water, decompose slowly, as Bywater acutely remarked. The action is very slow, so that there is nothing absurd in supposing that chemical action may go on for eight years, or more, as in the case of my tubes of china clay and water. The silicic acid dissolved out of the clay promotes rather than prevents pedesis.

In attempting to explain the exact *modus operandi*, we

cannot rest, as hitherto, upon experiments. We can only speculate that the action upon a minute irregular fragment will never be exactly equal all round. Differences of potential will exist, owing, in the first place, to the non-homogeneous nature of the particle. In accordance with all known laws of electro-dynamics, motion of the particle will result, which, by exposing new points of the particle to the action of fresh liquid, will alter the potentials, and lead to motion in a new direction. In order that a particle shall rest motionless in a non-conducting fluid, it must be in exactly equal chemical and electric relation to the fluid on all sides. That this should happen is almost infinitely improbable. A condition of unstable equilibrium within limits is the result—a condition partially analogous to that of a ball suspended on a jet of water, which is always falling off one way or another, but is always brought back again.

Having once begun to speculate it is easy to go a little farther, and to point out that there is probably a close connection between pedesis and the phenomena of osmose so carefully investigated by Graham. The connection is probably that of action and reaction; for if a liquid is capable of impelling a particle in a given direction, the particle, if fixed, is capable of impelling the liquid in the opposite direction by an equal force, just as a steamboat, if not allowed to move forwards, will throw the water backwards. The earthenware jars used by Graham in many of his experiments consisted of silicious minerals, which would give pedesis in great perfection; and Graham came to the conclusion that whether the septum consisted of earthenware, animal membrane, or other substance, the motive force arose from chemical action upon the matter of the septum. The membrane was always corroded to some extent—a fact which quite accords with my explanation of pedesis. The solutions employed by Graham were also such as would admit of this view, containing usually about 1 per cent of the salt. It is obvious that if a pedetic particle be in contact with precisely the same kind of liquid all round, there would be no determinate direction of motion either of particle or of liquid; but if one end of the particle be immersed in different liquid from that which surrounds the other end, a tendency to motion in a determinate direction will result, and this would be the condition of things in the porous earthenware jars of Graham. I do not pretend, however, to explain the exact *modus operandi*, which must be left to some one well versed in electrical science, but it is probable that the ultimate

explanation must take into account the remarkable phenomena of electric osmose investigated by Wiedemann.

It was first observed by Porret that when the poles of a battery are placed in two portions of water separated by a porous division, not only is some of the water decomposed, but another and far larger portion is impelled towards the negative pole. Wiedemann found that for 1 part of water decomposed, 5000 parts were transported through the porous septum. This impulsion is greater as the electric resistance of the liquid is greater, and ceases altogether when sufficient acid or salt is added to render the liquid a good conductor. There is an obvious analogy to the circumstances of pedesis, except that the energy is derived not from the septum, but from a cell. If these surmises should prove correct the phenomenon of pedesis will acquire great importance, because it was believed by Graham and others that osmose plays a large part in the functions of life, probably causing the motion of sap in plants and of juices through the walls of animal cells and vessels generally. It is certain that the solutions of gum, albumen, &c., which permeate the vessels both of animals and plants, are generally well fitted for the production of pedesis. From this point of view it is much to be desired that the inquiry should be extended, so as to include the relations of organic particles to solutions both of salts and of organic substances. I have hardly been able to enter upon this part of the subject. The motion of particles of gamboge has already been mentioned, and other organic paints, such as sepia and indigo, give like results. The yolk of an egg rubbed up in water showed intense pedesis. These motions are far from being prevented when the medium is a solution of albumen, gum, glycerin, gelatin. Some experiments seemed to show that, in the presence of these organic substances, small quantities of acid or alkali would not prevent pedesis. Observations of this kind duly extended might have a most important bearing upon many motions, such as that of cyclosis observed in vegetable cells, or even upon the whole subject of the motion of fluids in vegetable and animal organisms. I tried a few experiments with the sap of a tree, and found that gamboge rubbed up with it gave *a perfectly boiling appearance*. Other organic mediums—such as alcohol, ether, amylic alcohol, chloroform, spirits of turpentine, wood spirit, &c.—seemed to arrest pedesis; but they ought to be much more carefully tried with different suspended particles, and in mixtures of different proportions with water.

It is very interesting to follow out the effects of this pedetic motion from the geological, physiographical, and economic points of view. Its importance is immensely great, and it is not too much to say that the surface of the globe would be very differently shaped were the phenomena otherwise than they are. The disintegrating and carrying power of pure water mainly depends upon the pedetic motion, which, as we have seen, prevents rapid aggregation and subsidence. Now, the rain-water which falls upon the surface of the earth is sufficiently pure to produce pedesis in considerable perfection. Every little stream and pool of muddy water bears evidence to this effect, and fresh water as it flows down the rivers, and even into the sea, continues to hold great quantities of silicious particles in suspension. But no sooner has the fresh water become mixed with salt than the pedesis is destroyed, as I have ascertained by direct trial with sea-water, and the particles subside rapidly. Thus we account for the wonderful clearness of ocean-water, and the fact that the ooze from the ocean bottom consists almost entirely of organisms produced out of the substances contained in solution in the salt water. In the same way we may account for the peculiar clearness of alkaline solutions, with which I have often been struck. A pan of brine at a salt-works, for instance, if allowed to stand undisturbed for a little time, becomes as clear as crystal. This is due to the perfect subsidence of all foreign particles, in spite of the greater specific gravity of the solution which would tend to buoy them up. In the same way we may explain the dark blue colour of the Dead Sea, which is due to the clearness of the dense solution.

It now becomes easy to understand the peculiar turbidity of glacier streams, for the water proceeding from melting ice is in the most perfect condition for producing pedesis. I have examined water derived from melting snow, and find that the contained air is not sufficient to diminish the sustaining power much, and I have shown that the suspension of clay is continued longer in ice-cold water than in warmer water. That the particles derived from glacier erosion exhibit pedesis in a high degree is evident from the silicious nature of the rocks eroded, but I have confirmed this inference by direct trial upon the sediment of glacier streams both in Switzerland and Norway. As the water from a glacier becomes gradually warmed and aërated, the particles subside and the water becomes clear; but there is some difficulty in understanding how this process is carried out with sufficient rapidity to produce the clear blue water of

the Lake of Geneva. It is well known that for domestic purposes water ought to be well aërated, so that the water from a pure rivulet is better than that taken direct from a mountain bog. This is at least partly due to the decrease of the suspensive power by the dissolved air; and the brilliant crystal clearness of well-water is probably to be explained by the mineral salts held in solution, which favour the throwing down of all sediment.

III. ENERGY AND FEELING: ALTERNATE AND MUTUALLY CONVERTIBLE AFFECTIONS OF MATTER.

By W. S. Duncan.

IN the philosophy which is the offspring of modern science there is believed to be but one substance with twofold properties or affections. That substance is matter, and its twin properties are energy and feeling.

The present position of physico-psychology, however, is logically imperfect, inasmuch as it places an impassable gulf between the two manifestations of matter—energy and feeling. These two affections, it is said, lie side by side, and in parallel lines. Energy never once begets feeling, nor does feeling ever beget energy; they never even affect each other in the slightest degree. Language of this import has been used, even quite recently, by some of our highest scientific authorities and by clear thinkers, though admitted by these authorities to be not unattended by difficulties.

But a philosophy such as that is not only attended by difficulties, but is so illogical, so discordant with human language and experience, that I feel sure it must ultimately give place to one much more consonant alike with sound logic, scientific truth, and human experience. First, what does experience say? Do we not say, when we wish to explain our actions, "Such an idea, or such a feeling, was my motive for so doing,"—meaning that the subjective motive preceded, and was the cause of the physical action? Do we not know that most of our actions are aimed at subjective results, as so much pleasure or so much pain, rather than at

mere physical or material well-being? Has not the ethical basis of the "*happiness* of mankind" been approved of by the highest scientific authorities as the best principle of moral action? Why, then, should mere physical energy aim at *subjective* results unless it can *cause* them?

But Science replies, "The doctrine of conservation of energy and the fundamental difference between energy and feeling exclude the intermixture or alternation of these two affections of matter." If any theory involved the rejection of the doctrine of conservation of energy, it would of course condemn itself, for nothing in Science is more satisfactorily established than that doctrine. But this objection could not apply to a theory which described energy and feeling as alternate and mutually convertible affections of matter; for if energy merged in feeling, and feeling in energy, no energy would be lost, and no new force imported into the realm of Nature. Spontaneous origination of energy or feeling would be both alike unknown. The objection that energy and feeling are fundamentally different is at first an apparent obstacle to the acceptance of a theory of alternation. But this objection is only valid if both aspects of matter are viewed from *different* standpoints, the one objectively, the other subjectively. View both from the same standpoint, and the difficulty vanishes.

I, a part of matter, experience that alternation of feeling and action as if my actions proceeded from my feelings and my feelings from my being acted upon; why may it not be so with every other part of matter, as well as that I call "me"? To feel and to energise seem both to be very much akin, if we regard matter as a living substance, and exclude from it the old notion that it requires something immaterial to move it and something immaterial to feel its motion. But it is also objected that the moment a part of matter is moved by a neighbouring part, the former moves and feels simultaneously; that No. 2 movement is the true consequent of No. 1 movement; and that feeling is merely *concomitant* in time, and never intervenes between movements 1 and 2. Now, is this objection sound? Does not the swiftest known force take time for its passage? If so, we have but to divide the whole time by the number of parts taking share in a vibration, when we have the mathematical equivalent in time which elapses between the receiving and the discharging of an impulse by an elementary part, and we may still farther divide this time into the parts required for the duration of feeling and acting respectively by the elementary part. Now receiving energy takes

precedence of discharging energy, as is very obvious, since no force originates spontaneously. That the affection of matter in the receiving stage is somewhat different from that in the discharging stage is implied by the very term "receiving," being suggestive of passivity in contrast with the discharging stage, which latter stage alone answers to our notion of activity or energy.

Thus the fact of precedence in time between receiving and discharging energy removes the objection of apparent concomitance of feeling with energising states of matter, leaving time for the fact of alternation to transpire. Then the difference in nature between receiving and giving helps us to recognise affections of matter differing as much as do our ideas of the nature of feeling and acting respectively; while the possession by ourselves of powers to feel and act alternately as characteristic living powers helps us to understand how Nature, in part and in whole, may be endowed throughout, and perpetually manifest these twofold affections of feeling and energising in continuous alternation.

Let us consider what are the advantages of this theory of alternation of feeling and energising affections of matter.

First.—It agrees with human experience and language.

Secondly.—It seems logically more consistent than the theory of concomitance.

Thirdly.—It preserves the doctrine of conservation of energy, and agrees with the great laws of continuity and causation in all their manifestations.

Fourthly.—It requires no exclusive confinement of consciousness to ganglionic matter, to a central nervous sensorium, or even to organised matter at all, but places feeling in all matter at the stage of receiving energy.

Fifthly.—It does not involve intelligence while predicating sensibility of all matter, for the terms feeling and intelligence are far from synonymous, the former being simple and elementary, while the latter is compound or complex. But it teaches us to search for intelligence elsewhere than in man and animals: for wherever groups of force channels are organised, there groups of feelings are to be inferred,—therefore intelligence; for intelligence is the product of groups of feelings in one organised whole.

Sixthly.—In the event of discovering intelligence higher than man's the old polytheistic notions may not revive, inasmuch as those superior intelligences, equally with ourselves, must be subject to the great law of causation.

Seventhly.—Much of the mystery of adaptation in things, many of them removed distantly in time and space, seems

removed; for if feeling alternate with energy, what wonder that they should result in adaptations favourable to subjective as well as physical harmony or well-being.

Eighthly.—Nature is regarded in the light of the theory of alternation of energy and feeling, as a living whole, conscious as well as active; conscious because active, and active because conscious. How infinitely superior is it to the theory that regards Nature as the home of a sort of Siamese twins,—energy and feeling,—accompanying and complimenting one another, yet quite unnecessary the one to the other,—the theory of concomitance!

IV. THE GOLD AND PLACER MINES OF WICKLOW.

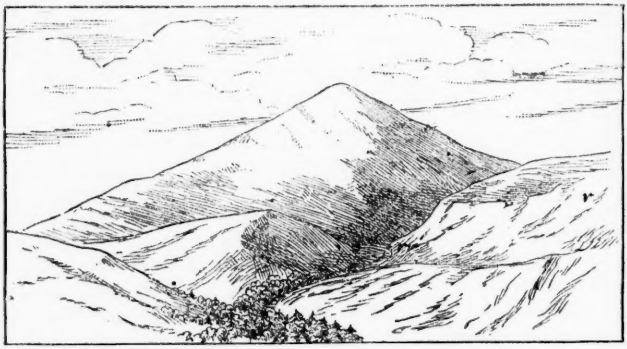
By G. H. Kinahan, M.R.I.A., &c.

LENIGAN and other English writers represent the Ancient Irish as perfectly ignorant and barbarous. Nevertheless, from the Annals, and the "finds" of gold ornaments and weapons in numerous localities, it would appear that at a very early age the use and manufacture of the precious metals were known to the natives of the country. That gold was much more abundant in Ireland than in England is shown by the fact mentioned in De Larne's "History of Caen," when, after the Norman Conquest of the British Islands, treasures were exacted from both to the exchequer of Normandy; the tribute exacted from England was 23,730 marcs of silver, but from Ireland 400 marcs of silver and 400 ounces of gold.

According to Dr. Joyce ("Origin and History of Irish Names of Places") the earliest known record of the manufacture of gold in Ireland was about 1000 years before Christ, when the monarch Tighernmas caused goblets and brooches to be manufactured by an artificer named Uchadan, who resided in Fercualan, or that portion of Wicklow which is now the Barony of Powerscourt. Besides, other monarchs are also mentioned who had gold manufactured, or who introduced the use of golden ornaments. On account of the abundance of gold in Dublin and Wicklow in ancient times

the inhabitants of this portion of Leinster were called Laighnigh-an-oir, or the "Lagenians of the Gold." as mentioned by O'Curry in his lectures (Lecture 1, p. 5.)

From the records in the Annals it would also appear that the ancient placer mines were principally situated in the Dublin and adjoining mountains. In these hills in recent times very little gold has been found. Now and then pieces of gold are picked up in Glenismole, or the upper portion of the Valley of the Dodder; and recently in Stephen's Green, Dublin, a small nugget was found in a load of gravel brought from the Dodder Valley. In some of the valleys eastward of Blessington, on the Liffey, are broken patches of ground that have an appearance like the sites of ancient placer mines; and farther south, workings of the ancient Irish



Croghan Kinshella. *r*, Iron mine; *rr*, Gold placer mines.

were discovered about thirty-five years ago, in the placer mines at the Hill of Lyra (*anglice*, junction or fork of two glens), townland of Knockmiller, nearly a mile S.W. of Wooden Bridge. In connection with these ancient works, under a depth of about 5 feet of meteoric drift, a black oak frame was found. It was 14 feet long by 10 or 12 feet wide, the shorter beams being morticed into the others about a foot from the ends, and on the outside of all the beams were carving of animals, some having men mounted on them. These consisted of "animals like mules with long bob-tails, others like goats or deer, and some of a nondescript character." The beams were cut up to make ground joists for the east wing of Wooden-Bridge Hotel.

The Lusceans, or the inhabitants of the Lusca or Earth-caves, must have been rather a primitive race, if we are to

judge from their habitations, that were rarely high enough for them to stand up in, and into most of which they had to creep through passages scarcely 2 feet high; yet in these caves most highly-wrought gold ornaments and weapons are not uncommon; while under some of the deep bog they also occur. Of the "finds" in bogs, those in the Bog of Cullen, at the junction of the counties of Tipperary and Limerick, are the most remarkable, as here were found not only innumerable golden articles, but also the various implements used by the goldsmith, such as crucibles, ladles, and the like, among the "corkers" or roots of the trees of an ancient forest.

As pointed out by O'Curry, this locality seems to have been the habitat for years of a race of goldsmiths, who carried on the manufacture from one generation to another in the wood there situated, long before the bog began to grow. O'Curry has tried to identify these goldsmiths with a race of artificers whose genealogy is given in the book of Lismore, who were direct descendants from Olioll Olum, king of Munster, and followed the trade from about A.D. 300 to A.D. 500; but the thickness of the bog over the ancient forest, among the remains of which the articles are found, would seem to suggest a far greater age.

The numerous and rich "finds" in the Bog of Cullen during the last two hundred years has made it proverbial in Munster and celebrated in song.

It would seem that after the conquest of Ireland by the English the existence of gold in the country was unknown or forgotten; but in recent years it was remarked that from time to time the natives of Wicklow brought up small quantities of gold to sell in Dublin. This did not create much inquiry till 1795, when a large nugget was offered for sale. The exact weight of this is uncertain, some say 21½ ounces, others only 18; while some authorities mention two large nuggets. Whatever its weight, on enquiry it was found that it had been picked up by a girl driving cattle over the ford of Ballinasilloge, in the stream now called the Gold-Mines River. This runs eastward from Croghan Kinshella to join the Aughrim River, and eventually the Ovoca, at Wooden Bridge.

The find of this large nugget caused a rush to the place, and for over six weeks from 600 to 700 people were working in the valley and neighbouring streams, till the Government, fortified by a special Act of Parliament, took possession, and established more systematic placer mining, under Messrs. Weaver, King, and Mills. The Government mining seems

to have been mostly carried on in the Gold Mine Valley, while at the same time adventurers were working in the neighbouring streams of Ballintemple, Clonwilliam, Knockmiller, Coolballintaggart, a stream on the other side of the hills running north from Croghan Kinshella, and, farther northward, at Mucklagh, Ballinagappoge, and Sheanmore, the last four being on tributaries of the Aughrim River.

The Government Works continued till May, 1798, when they were interrupted by the Rebellion, and were not resumed till 1801, after which time, for reasons fully stated by Weaver in the "Transactions of the London Geological Society," a level was driven into the east side of Croghan, while miles of trenches or "open cast" were made round its summit, in search of the "mother rock" of the gold: all these works resulted in failure, for, although numerous veins of quartz were discovered, not a particle of gold was found *in situ*. After the failure of the trials the Government were advised to abandon the works. Since then placer mining has been carried on by companies and private individuals, but not successfully; Prof. W. W. Smyth, in his Report states "partly it may be presumed from the rarity of the precious metal, and partly from the difficulty experienced in all gold-streaming or gold-digging regions of obtaining from the workmen the full produce of their labours."

The Government placer mining, both before and after the Rebellion, is said to have been remunerative, and the quantity of gold returned was 944 ounces, of a total value at the time of £3675; the assay of 24 grains, by Weaver, gave pure gold 22·58, and silver 1·43; while a second, by the Assay Master of the London Mint, Mr. Alchorn, gave, for the same weight, 21·375 fine gold, 1·875 of silver, and 0·375 of an alloy of copper and iron. The streamings carried on prior to those conducted by the Government are said to have been even more remunerative, and Sir R. Kane states that it has been calculated that over £10,000 were paid for the gold sold by private individuals.

The minerals found with the gold, as recorded by Weaver, are magnetic iron ore (sometimes in masses over half a hundredweight), titaniferous iron, specular red and brown iron-ores, pyrites, tin-ore and wolfram, manganese ore, garnet quartz, and lepidolite; to which Mr. Mallet has added platinum, galenite, chalcopyrite, molybdenite, sapphire, topaz, zircon, and spinella ("Journ. Geol. Soc. Dublin," vol. iv., p. 271). This observer seems to have come on an extraordinary prolific "run," as he records 3·5 lbs. of tin ore from 150 lbs. of sand; while all other observers have found this valuable ore

only in small quantities. It may be mentioned that in the old working, in the wood, tin ore was more frequently found than higher up, or to the south-west, where it is rarer. Tin ore is also recorded for the working at Ballinagappoge, one of the tributaries of the Aughrim River.

Most of the gold is in "eye-sills," or small particles; in some places, however, "large gold" or grains occur, while here and there are nuggets, ranging from 21·5, the larger, to half an ounce; in one placer, a little N.E. of Ballinasilloge ford, many nuggets averaging from 3 to 5 ounces, one being 11 ounces, are said to have been lifted. In Ballintemple there was a great deal of gold, but it was nearly all in "eye-sills." The placer of Knockmiller is said to have been very productive, "large gold" and "eye-sills" occurring together, while in Coolballintaggart "large gold" was principally found.

In the "Elements of Geology," recently published by Prof. Le Conte, of the University of California, we learn that the California placers are below slopes on which there is always an outcup of an auriferous quartz vein. In Wicklow no auriferous quartz vein has been discovered, but in all cases the placers are below the slopes on which certain iron ore lodes crop out. On this account all the different observers seem to believe that there must be relations between these lodes and the gold in the placers; furthermore, fragments of these ores always occur in the sand associated with the gold. However, in none of the lodes above the placers has gold been found, although farther north-east, west and east of the Ovoca, it occurs in the gaussen at Ballymurtagh and Cronebane, and in the remarkable mineral Kilmacoite, or "silver-blende," at Connarry.

The major portion of the gold is abraded, and apparently has been drifted to its present site; but some nuggets and many of the eye-sills are frosted, as if they had grown in the drift, similar to some of the gold found in the "deep placers" of California; other pieces are *attached to quartz*, especially in the vicinity of the "Red Holes," a swampy patch at the mearing of Ballyvally and Ballinasilloge, as if somewhere in the latter townland there is a still undiscovered auriferous quartz vein. It must, however, be allowed that the chances in favour of the latter are small, as hundreds of tons of the quartz erratics were brought to Ballintemple, and crushed, without even a particle of gold being got to repay the trouble and expense.

In the Red Hole Mines, at present being worked by Mr. F. Acheson, we learn that the surface-accumulations in

the ravine consists of—above meteoric drift, under which is water-formed drift, and at the base of the latter in places is the auriferous or "black sand." The black sand is found in "runs" or lines, and these occur in channels or slight hollows that have been denuded or worn out along nearly parallel lines of dislocation, or master-joints, in the underlying Cambro-Silurian rocks. The miners know when they are coming to a "run," as the lamination of the "bottom rock" is "crumpled," as the twisting and breaking-up of the rock adjoining the fault or dislocation lines is locally called. In each section of the valley these breaks have general bearings, so that when the direction of one run is known all others are nearly parallel. One set of parallel breaks may, however, be crossed by another, and at the junction of the two systems the channels are deeper, and consequently in such spot more gold collects than elsewhere; so that on a map of a placer the rich spots occur somewhat like the corners of the squares on a chess-board, only more oblique to one another.

In the Red Holes Mine the surface of the "bottom rock" in general is ground smooth, as if a rapid torrent had ran over it for years. In the overlying water-drift all the rock fragments are abraded, and rarely—even on the Gabbro, quartz, and other hard rocks—was a trace of ice-work detected; but a few ice-dressed fragments do occur in the higher meteoric drift. It would appear that when the ice was melting off the Wicklow hills great torrents were flowing down the different ravines, and when the ice had all melted and the water-supply was gone the torrents dried up, while subsequently the marginal cliffs of the ravines weathered into slopes, their detritus forming the meteoric drift now found above the water-drift.

All the modern mines in the neighbourhood of Croghan Kinshella are "shallow placers," the deepest being less than 30 feet, while no deep trials have been made. In nearly all other gold regions the precious metal has been worked not only in shallow but also in "deep placers," and, if we may reason from analogy, it appears probable that there are vast supplies of unknown auriferous sands under the deep river and estuary accumulations in the flats at Wooden Bridge, and other places in the valleys of the Ovoca and its tributaries.

V. ON THE RELATION OF MOISTURE IN AIR TO HEALTH AND COMFORT.*

By ROBT. BRIGGS, C.E.,
Cor. Mem. Am. Inst. of Architects, &c.

IT may be accepted that the most pleasant condition of the air, in our portion of the globe, will be found to exist on a fair day in early summer, when the temperature ranges from 62° to 68° F., and the moisture present in the air is from 85 to 80 per cent of saturation. On a day like this no thought of the weather is taken, and the passage of the conversational compliment of a "fine day" becomes a needless reminder, to be accepted without discussion or thought. Admitting this proposition as a fact, it is the purpose of this paper to show that in our climate this summer condition of relative heat and moisture is not desirable, or even attainable, at other seasons, in the ventilating or heating of occupied places. And in presenting this view to the American Society of Architects I do so with a full knowledge that it does not accord with the opinion of most American writers on the subject of ventilation, who have derived their information and their arguments mainly from the study of English and French books, and have only endeavoured to reconcile the data found in these to American wants and practice. Even Wyman, who is by far the most original as an observer, as well as the most thorough as the collator of information from all sources, hardly makes the distinctive effect of our low temperature, combined with a comparatively lower dew-point, sufficiently evident.

Except one investigates the relation of moisture to temperature of air in the two countries, it is impossible to reconcile our facts with the statement of good foreign authorities,† that 56° is comfortable, and 62° is warm, in

* Paper read before the Am. Institute of Architects, at Boston, Oct. 18th, 1877. From the Journal of the Franklin Institute.

† The comfortable warmth of air indoors is given by various authorities, as follows:—PECLET, Traité de la la Chaleur, gives 15° C., 59° F. MORIN, Etudes sur la Ventilation,—for nurseries, schools, &c., 59°; hospitals, 61° to 64°; theatres, assembly halls, &c., 66° to 68°! TREDGOLD, Principles of Warming and Ventilating, &c., 56° to 62°. REED, Illustrations of the Theory and Practice of Ventilation, 65°. HOOD, Treatise on Warming Buildings, &c., inferentially 55° to 58°. PARKES, Manual of Military Hygiene, 48° to 70° (this author has encountered the difficulty of naming a fixed temperature, and avoids the issue). BOX, Practical Treatise on Heat, 62°. Others might be quoted, but these are amongst the best authorities on Heating and Ventilation.

living rooms in mid-winter; while the American shivers with cold at 70°, and is not over-warmed at 76°, in the apartments of his own dwelling, although clad in the thickest of underclothing. Investigation, however, shows that the deprivation of heat from the person is more due to evaporation from the lungs or throat, and from the skin, than from heat otherwise dispersed, whether carried off by the breath, imparted to the air, or radiated to surrounding objects. And further investigation will show that the hygrometric state of the air has so much effect in inducing or retarding evaporation as to make 56° F. in the West and South of England, in Ireland, and in Normandy, sensibly as warm as 80° in Canada or Minnesota at the same season. A brief statement of the difference of climatic condition of England and of America may show why we cannot import English theories of ventilation and heating, and apply them at once, without modification, to American residences. The English climate affords nearly eight months in every year when the thermometer ranges between 40° and 60° in the shade, with a dew-point so high that it is a pleasure to exercise in the invigorating air; one month of 60° to 80°; and three months from 25° to 50°; there being no term, except a part of the one month of heat reaching to 80°, when any person cannot, with suitable clothing, enjoy the open air. While in America there is scarcely one month (or thirty days) out of the year having an average temperature of 50° to 75° (which temperatures, from the difference of dew-point, correspond sensibly with 40° to 60° in England); and there are three months of 75° to 90°; three months of 30° to 50°; and five months of excessive variation of temperature of from 0° to 50°. During the three hot months, and also during most of the five cold, open-air exercise to those whose avocations are within doors is, if not impossible, at least very uncomfortable, however clad or covered. Anyone who is called upon to endure the fervid summer heat, or who can habituate himself to the inclemencies of our arctic winter, will not suffer great discomfort, nor experience much injury to his health therefrom; but the weak and tender—the merchant from his counting-house, the student from his closet, the workman from the shop, the women and children of the house—cannot acquire the endurance or the habit, and must shelter and protect themselves.

This preamble to the subject has been intended to impress the fact that its consideration must be on its own merits, and not through the light thrown upon it by general writers, that its investigation shall be original from physiological considerations, and not based upon authorities.

§. Comfort, if not existence, depends upon a constant loss of heat from the person. The internal natural warmth of the body is very nearly 100° F., regardless of the heat of the external air; and the personal comfort which proceeds from the temperature and humid condition of the air, proceeds from the cooling effect which must then occur with constancy and regularity, and yet not so fast as to produce the sensation of cold. The origin of the natural heat is well established. There is inhaled by each adult in comparatively still life, each three to four seconds, from 30 to 40 cubic inches of air, at such temperature as may exist at the place, with extreme differences of temperature ranging from −40° to +140° F., and with extremely variable proportions of humidity, from the point of saturation on the one hand to that of nearly an anhydrous air on the other. The practical extremes in our country are from little below zero to about 100°, accompanied also with great variation of humid condition. A portion of the oxygen of the inhaled air is consumed in the system, and the exhalation, which follows each inhalation, emits about 4 per cent of carbonic acid and $1\frac{1}{2}$ per cent of vapour of water. Two or three grains of carbon are consumed in the system each minute, giving out $3\frac{3}{4}$ to $5\frac{3}{4}$ units of heat, the unit of heat being the equivalent to a pound of water heated 1° F. It is the dispersion of this heat which establishes the sensation of comfort. Modern theory has established the convertibility of heat to work or power, and some portion of the heat evolved by the air of respiration will have been converted into labour or effort, but far the greatest portion will have been utilised in preserving the temperature of the body from the losses by evaporation of moisture, by conduction, and by radiation. One portion of the loss is readily estimated. The breath is inhaled at whatever temperature and humidity may subsist at the place, but is exhaled at all times at 90° (when the temperature of the air is not above that degree), and it is saturated with moisture at that temperature. If it is supposed that the temperature of the external air is 62°, and the dew-point 54° (= 65 per cent humidity), from 0·35 to 0·56 unit of heat will have been expended in evaporation of moisture in the lungs and throat, and 0·10 to 0·17 unit of heat in imparting heat to the exhaled air each minute. About three and a half times as much heat will have been expended in supplying the moisture as in heating the air. The loss of other portions of heat cannot be as definitely estimated. It is evident that it must mainly be dispersed from the skin, and it is pretty certain that a large, if not much the larger, portion must pass off in

insensible perspiration, which will be greatly affected by the condition of humidity of the surrounding air. Here clothing becomes an important element. We protect ourselves against the inclemency of winter, or the heat of summer, by coverings more or less non-conducting or non-radiating, leaving but a small portion of the person unprotected by direct exposure. An almost instinctive preference is given by all people, of all times, and at all places, for porous clothing; even the skins which clothe the inhabitants of the coldest regions, although quite impervious to moisture and to currents of air, are very open for the passage of vapour of water, or of diffused gases. Evaporation, and consequent cooling of the skin, takes place in great measure, or is influenced by the relative vacuum which the quantity of vapour present in the air establishes. The transfer of vapour is then one of diffusion, and follows the law of diffusion of gaseous bodies. A partially anhydrous air, external to the clothing, is a partial vacuum to the vapour of 90°, existing in ducts of perspiration, and this vapour rushes towards the vacuity without encountering the resistance of any circulation, and meet no considerable obstacle in the porous coats and overcoats. It is in this way possible to explain why, in mid-winter, with the room from 65° to 70°, heavy underclothing is not only endurable, but necessary. The overcoat may be removed on entering the well-warmed house, but no discomfort follows from the retention of warm garments that, with a summer condition of air of the same temperature would be oppressive. We sleep in rooms which, if not warmed to the full heat of our living rooms, have yet the "temperate" point of indication by the thermometer, and in this case enjoy a pile of bed-clothing, which would be suffocating in weather of the same natural temperature. The American requirement of comfortable drawing-room clothing is strikingly different from that of England. The ladies' English drawing-room dress is an impossibility in America. Even the rigorous laws of fashion fail to conform themselves in this case, and yet our American drawing-rooms are *hotter* than the English ones.

What proportion of the heat generated by formation of carbonic acid to be dispersed from the body after taking out what is abstracted by exhalation and by labour of work or animal life is expended in vaporisation of water is of course doubtful. Some authorities give 2 to $2\frac{1}{2}$ lbs. of water to be evaporated each day by insensible perspiration. These quantities would give (nearly) 2000 to 2500 units, or $1\frac{1}{2}$ to $1\frac{3}{4}$ units of heat per minute, and, together with the

quantity of heat dispersed in breathing (on the previous supposition), account for one-half of the heat produced; leaving one-half the heat to be dispersed in work, life, or conduction to air, or radiation to other bodies. These authorities quoted are, however, English, and it is uncertain what quantity of moisture is evaporated from the skin in heated rooms in mid-winter in our climate.

It is probable that in still air, with the person in repose, the transfer of heat, either from the person or the clothing, whether from radiation or from conduction, is nearly equal; but in any current of air or movement of the individual the effect of conduction will much exceed that from radiation. It should be remembered, however, that a current of air always exists about any person. The comfortable temperature of the air being lower than that of the person, there is established, by the heat imparted to the air by the person, an ascensional current surrounding and enveloping him, sufficiently defined to be measurable by a delicate anemometer, which is effective in augmenting considerably the convection of heat over what would occur in entirely still air. Assuming any comfortable temperature for air between 60° to 80°, the exhalations of breath, by virtue of extra temperature and the presence of vapour to saturation, notwithstanding the addition of some carbonic acid gas of greater density than that of the air, are still so much lighter than the air as to ascend at once after the directional impulse from the mouth or nostrils will have expended itself, which, when the act of breathing has its normal force, and is not made violent by running or exertion, occurs within two feet.

In all cases the sensibility to loss of heat, whether from the breath as exhaled moisture or heated air, or from the person as evaporation from the skin, or as conduction to air, or by radiation to cooler objects, this sensibility, I say, varies in the several regards with different persons, with different races or nations, and, above all, with the habits from business pursuits or occupations, or the customs or fashions of the place of living; any of which causes may and will have established a *régime* in each individual, and their comfort will depend upon conformity thereto. The occupation, business, or habits of individuals as regards their labour or exercise, both when at labour or exercise, or when at rest, cause much discrepancy in demands for heat. In the coldest and driest day, few young persons can fail to warm themselves to the point of comfort in skating; many of the trades demand special temperatures for the workmen,

some requiring special temperature and moisture condition of the air for the work, to which temperature and condition the workmen must conform.

There are three means provided for the healthful dispersion of animal heat into the atmosphere; the first is radiation to surrounding colder objects; the second, conduction to the atmosphere, which, for comfort, must be sensibly cooler than the body; and the third is evaporation from the moist surfaces of the lungs, throat, and the roots of the pores of the skin. The first of these means, to the clothed person at least, is comparatively ineffective, while the relative quantities of heat which may be eliminated in any given time or locality, by the two last, will probably be found nearly equal in an atmosphere of about 70° temperature and 65 to 70 per cent. of humidity. In all cases of *excess* of animal heat, the animal, and mankind as an animal, find relief in evaporation of water secreted in the system, showing that vaporisation is the ultimate means of dispersion of heat.

Even the races of animals exhibit diversity of natural methods of dispersing the surplus animal heat. Thus the dog obtains relief through the breathing functions, and extends the surface of evaporation by exposure of the tongue, while the horse breaks out into profuse perspiration of the skin.

The relation of what is indicated by the sense of cold or warm to definite temperature with varied proportions of humidity may be examined at this stage of the argument. Considering a nearly saturated atmosphere, it will be found that its effects differ with the temperature altogether. Such an atmosphere at from 35° to 50° is found to be intolerably chilly, and although evaporation may be checked, and this source of loss of heat be removed, yet the conductive and radiating value of the vapour in the air is now elevated enormously. The cooled surface of the cuticle absorbs the natural heat of the skin, and represses the evaporation of secretions almost entirely. An actual transfer of heat from the skin to the vapour in contact with the surface occurs, the superheated vapour no longer rushes away from the skin in search of that vacuum, which is the accompaniment of a usually low dew-point, but merely transfers its heat to the next particle of cold vapour, which is packed in convenient juxtaposition to receive it; or else an actual movement of the heated vapour effects a circulation or current which brings a new cold particle to receive a new increment of heat. In an atmosphere of this nature the exhaled

breath and the exhaust steam from the workshops evolve a cloud of apparent vapour, which must condense in cooling as the air absorbs its heat, for the saturation of the air forbids its absorption as an invisible gas. A Scotch mist of 36° (which is only a supersaturated air with vapour in excess at a slightly higher temperature than the air) penetrates clothing, and reaches every part of the person with distressing frigidity.

Passing upwards in the scale of temperatures from 50° to 65°, the point of equilibrium of cooling action by conduction or radiation of vapour in the air, with supply of heat from checked evaporation of the skin or lungs for attainment of comfort, seems to be reached. Perhaps the most healthful, or at least most stimulating, atmosphere for human breathing is found within these limits, when of natural and continued existence, so that within and without the doors the same condition exists, and the *régime* of the bodily system is not disturbed from hour to hour. This, if not the ruling climatic condition of English life, at least is the presumed theoretic standard of English writers. Mental or physical exercise alike, either separately or in conjunction, are supported by this condition of the atmosphere to an extent which no inhabitant of the frigid north or enervating south can imagine.

Some curious physiological phenomena accompany this atmospheric condition, one of which is the possibility of use of stimulants of the milder nature (wine and malt liquors), in quantities which would be immoderate in our climate. With the comparative cessation of cutaneous evaporation, it seems as if action of the alcoholic ingredient of the liquors were much changed and rendered more stimulating and less intoxicating.

From 65° to 80° a saturated atmosphere is sultry and oppressive. The surplusage of heat cannot be removed by conduction, and the natural effort of the system is to induce evaporation. The least physical effort produces, in the healthy person, abundant sensible perspiration, and the cooling effect of evaporation of a heated surface of water into a cooler air is the natural remedy. The lassitude and enervation of this step in the scale is eminently unfavourable to mental as well as to physical labour.

Above 80° a saturated air becomes burdensome; it is even questionable if life could be prolonged in a saturated air of 90° to 100°, and it is certain that at some point, not much above 100°, suffocation would ensue when any exertion should raise the animal heat above its normal degree. The

deaths in the Black Hole of Calcutta were the result of excess of moisture, rather than of heat or want of air *per se*. There are travellers' tales of regions on the Red Sea, and near the mouth of the Persian Gulf, where men cannot breathe in summer for the heat combined with moisture.

In the same way that the effect of a nearly saturated atmosphere has been examined, that of a very dry one may be investigated. To the sense of feeling all air may be said to be dry below 35°. The small amount of vapour present, and possible to exist as vapour below this point, reduces the conductivity so that the chilliness, to a great degree, disappears, even in a saturated air. Yet even here the cold-producing effect of a high dew-point is felt in a wind, so that from 15° to 35° the N.E. wind of our Eastern States is a very raw one. But, on the other hand, with a dry air from 40° below zero to the freezing-point, the immediate sensation of cold by the active man, well clad with porous clothing, is yet endurable, and with habit becomes almost pleasant. Still these temperatures are not those suited to civilised life, either physical or mental. As has been before noticed, we have in the Northern States about five months of the year when the temperature ranges from 0° to 50°, and consequently when our civilised avocations demand artificial heating. The winter climate of the Eastern, Northern, and Middle States is one of great vicissitudes, with extremes, both of temperature and of hygrometric conditions, following each other rapidly. In the North-Western States it seems that a somewhat greater uniformity of temperature and a much more uniform hygrometry exist during the winter months, but in the Middle Western States the irregularities appear to be as frequent as in the Eastern States. Except that the length of the winter season is a little cut short, and the excessive cold is a little alleviated in the southern portion of the belt of country I have designated, much the same phenomena of climate exist all through the States north of the 40th parallel of latitude. Throughout this territory it has become recognised that the minimum temperature of comfort for heated and ventilated rooms can be stated at 70°, with an admitted—and generally supposed inexplicable, if not unreasonable—demand for 75° to 78° in some localities and at some times.

§. Ventilation means a supply of fresh air to the occupants of a house, workshop, or meeting-room of any kind; and, as a final result, the quantity of such supply needed

to attain the desired purity is from 40 to 60 cubic feet of air for each person each minute, where the contents of the rooms can be considered as furnishing a portion of the supply when occupancy is only for a part of the time. Much of the air may not be supplied through the heating apparatus. In cold weather, when the levity of the heated air within a building, compared with the colder air on the outside, produces a great pressure of outer air near the ground, the leakage of air at cracks of the door and window-frames, at the top of the building, and its replacement by colder air through similar apertures at the bottom, furnish a much larger volume of air than is generally supposed. The strong winds also seek such leaks. Some permeability of walls, even of boards well painted, is available for the diffusion of vapour and of gases in a measure. So that the proper quantity of air to be supplied by an apparatus becomes a question to be considered, in each instance, on its own merits. But the fact still remains, that for each adult or child in health, 40 to 60 cubic feet of fresh air must be estimated as provided, either by arrangement or surreptitiously, for each minute they may occupy a room or place, although not necessarily during each minute of the day and night. This fresh air must be derived from out of doors.

Accept, for the sake of argument, the average temperature and dew-point of Philadelphia, in January, February, and March, of 1844, as reported in Prof. Bache's meteorological and magnetic observations. The mean temperature of those three months was 34°, with an average dew-point of 25°; barometer 30 inches, from hourly observations, giving 68·8 per cent of saturation. Using Guyot's Psychrometrical Tables, Regnault's data, 1·57 grains of aqueous vapour exist in a cubic foot of such air. These, in Philadelphia, at this season, are the unquestioned properties of the air from which is to be furnished the fresh air of ventilation. If heated to 70° without increment of moisture, the dew-point remains unchanged, and the same 1·57 grains of moisture appertain to the enlarged volume of air, now increased 8·2 per cent by expansion. The hygrometric condition of this air is but 1·44 grains per cubic foot, or but 18 per cent of saturation. The summer hygrometric condition of air can be derived from the same source. The three months of July, August, and September give 71° average temperature, with 60° dew-point, or 68·3 per cent of saturation. Suppose we take the 68·3 per cent, and consider it the proper condition for the air of ventilation at 70°; it then follows that

5·46 grains of moisture should accompany each cubic foot of air in winter. One more step in calculation, and I have done. A cubic foot of air at 70° weighs 0·074 lb., and if it has been elevated in temperature from 34° or 36°, then 0·635 unit of heat will have been expended in warming the air. Again, if the amount of moisture present in this cubic foot of air has been increased from 1·44 grains to 5·46 grains = 4·02 grains, then 0·612 unit of heat will have been expended in evaporation of water to supply the moisture to vaporise the air. The quantity of heat for warming very nearly corresponds with the quantity of heat for vaporisation! The tension of vapour of the external air at 34°, with 25° dew-point, is 0·155 inch of mercury; that at 70°, with 59° dew-point, is 0·515 inch of mercury. It follows that the pressure tending to diffuse the aqueous vapour from the "hydrated" room to the external air would be 0·365 inch of mercury. The vapour itself, within the room at the same time, possesses but 1-48th the tension of that of the air present, and hence, as it is endeavouring to escape under the pressure of about 25 lbs. per square foot (which corresponds to the 0·365 inch of mercury column), it becomes evident that it would diffuse through cracks, outlets, and even through the passages for supply of fresh air, with great rapidity, and that this ratio of saturation is practically impossible to maintain in any *ventilated* room, or even in any room whatever, as ordinarily enclosed and built.

These estimates and considerations show fairly what would result from the attempt to produce an artificial summer climate in the houses of our Northern States in winter; but while the futility of the effort in its complete accomplishment is made evident by them, it by no means follows that some degree of hydration of warmed air is not the requisite of health or of comfort, and the question recurs—What proportionate hydration is needed for these ends?

§. It is with some reluctance that I refer to the "Theory of Ventilation." The past eighty years have witnessed the growth of chemical science, which, after passing through numerous stages of development, as witnessed by the different nomenclatures, has finally reached the point that only the chemists of continual study can comprehend it, and at which point he who knows most about it is the least satisfied with its present condition.

But thirty or forty years since chemistry was supposed to have unlocked the mysteries of matter, and, by the extension

of the simple rules applicable to the gaseous and metallic elements, it was thought that the cause of disease or health was to be discovered. Careful observers then examined, from the chemical standpoint, the constitution of the air, both fresh and vitiated; and writers, with good logical conclusions, enunciated a *theory*, by which it was made evident that chemistry had uncovered the root of disease, and carbonic acid gas was the fatal cause.

The real facts are these:—An adult in still life inhales each minute about 480 cubic inches of fresh air, and exhales 488 cubic inches of vitiated air, of which vitiated air about 4 per cent is carbonic acid, and from which about 19 per cent of the oxygen originally supplied by the fresh air has been abstracted; the original quantity of vapour in the fresh air at mean temperature and hygrometric condition (62° and 65 per cent) will have been increased from 1·1 to 3·08 grains.

Carbonic acid gas was made the scapegoat. It killed dogs at the Grotto del Cane, as was happily exhibited to numerous travellers in Italy at that time—and both before and since, the same unfortunate dog serving to be killed, to the satisfaction of admirers, that had been resuscitated the day before, after the visitors' backs were turned. It was heavier than air, and in some conditions of temperature would not so readily diffuse, but form a layer of distinct gas, like water beneath oil. Altogether it answered the conditions of hypotheses, and it was decided to be vile, deleterious, poisonous. To be sure we devoured it in bread, and drank it in beer or aërated waters, but then the poison was to the lungs, not to the stomach! This theory found promulgators in the lecture-rooms and advocates in the household thirty years ago, and has become to-day the *traditional* belief of the middle-aged and elderly. If a room is hot or close from excess of temperature, or from a crowd of occupants, carbonic acid gas is the difficulty; if malaria is developed in a jail or hospital, or typhus or scarlet fever exist in the dwelling, carbonic acid gas in excess is the poison. "The gas is heavier than air, and must necessarily sink to the floor, where all the air of vitiation will be found." These notions continue to have advocates and supporters to the present time, and the popular lecturer or writer gives a half assent to them to secure the favourable opinion of audiences or readers. But step by step, during the past thirty years, it has come to be perceived that the causes of disease are not to be found with organic matter, and carbonic acid has been removed from its elevated place in

ventilation, with the fullest admissions that in the quantities ever present in the living rooms, except by accident, it is quite harmless; and, finally, its presence has been accepted as merely a measure of other more dangerous vitiations, in that as it is a definite product of respiration, and as the proportion present in any room, at a given moment, can be ascertained with tolerable exactness, an indication can be derived thereby of the extent of organic vitiation with some degree of certainty.

The unquestioned theory of malaria, the meaning of which word can be extended to embrace diseases arising from deficient or defective ventilation, to-day, is organic vitiation, and the probability of this theory holding its place in future is, I think, a very fair one. The exhaled and exuded vapour from the human body is known to be laden with organic matter: much of this organic matter is within the range of the microscope, by means of which the local derivation of many particles can be determined; but some of it is in the form of effluvia and odours, which pass the limits of visual observation in the smallness of the atoms, notwithstanding such effluvia or odours are decidedly perceptible to the sense of smell. With a dry external atmosphere and a reasonably free ventilation the exuded vapour and the organic matter pass away, or are diffused as rapidly as supplied. *It should be remarked that the organic matter appears mainly to be in connection with the vapour in the air, and not to exist as a separate gas, diffused in the dry air when the vapour is removed by natural causes.* With an imperfect or insufficient ventilation the upper parts of rooms become filled with air, which will be found to contain a much larger proportion of moisture than the lower portions, and will be shortly found to be exceedingly offensive from the rapid decomposition which the exuded organic matter undergoes in a moist air. This will happen more frequently when the internal and external temperatures are about the same, and when it is so cold, raw, or windy as to require closed doors and windows, with only a small addition of heat, and when with these conditions the natural dew-point is high. These circumstances are in concurrence frequently in England, where probably 120 days out of 365 call for but small addition of heat in rooms, while they rarely exist with us, the climate of our northern United State not giving 30 days in any year of similar kind. The objection of *effluvia*, which forms the distinctive one in audience-rooms in England, and is so noticeable to the American visitor of such rooms, is replaced in our halls by a simple sense of oppression—a

mere feeling of discomfort—which, on the other hand, is particularly noticeable to the English visitor of our halls, who is apt to associate it with a supposed excess of heat.

But this organic matter of exhalation is still one step removed from malaria; it is only the ground of malaria—the soil on which a malarial growth will propagate; its *decomposition* is held to supply the means of fecundity to the germs of disease. In the warm air confined in the upper parts of rooms, with excess of moisture, it may undergo a rapid and somewhat fœtid decomposition; under such circumstances it is found to become offensive in six to ten minutes. With a smaller proportion of moisture, or when it is rapidly absorbed with the moisture by diffusion into air of American dryness, it does not decompose so rapidly, but is likely to be absorbed by any hygroscopic substance the air containing it may come in contact with. The walls of rooms—especially the porous plastering, stone, or bricks, and possibly the papered and painted walls—will take up the excess of moisture with its organisms, and give up at another time, wholly or in part, the moisture without them. There is a characteristic smell of walls of kitchens, cabinets, hospitals, jails, court-rooms, and similar permanently occupied places, which can be developed in intensity by simply holding the half-closed warm hand against the face of the wall, and testing the result by the sense of smell of the hand. In such instances of retention of the results of imperfect ventilation, the eventual propagation of disease is a certain one.

To the vegetable and lower animal growth the presence of moisture in the air seems a positive necessity; where it is absent they perish, or at least no longer grow or propagate. The drinking animal apparently suffers the least of injury and but little discomfort in the dryness of the air, of whatever temperature. The immunity from disease to the human race which accompanies the drier regions of the earth has been frequently remarked, and the fact meets general assent. The discomfort of the colder countries, even to the limits of the arctic regions, is one of cold, not of absence of moisture. The assertion of relative insensibility to cold air devoid of moisture is the common report of all travellers in such regions, while the dreaded malarial diseases of more genial lands do not exist in them at all. In the temperate zone, in countries or localities which possess the driest of atmospheres, with the least variation of hygrometric condition, mankind is most free from disease of all descriptions. The elevated dry and barren lands of

our midland country, from Minnesota southwardly, are the healthiest regions of the United States; and where, together with the *dry* atmosphere, some uniformity of temperature exists, as in Mexico,—where the height above the level of the sea reduces the sensible heat of the air which is usually found in that latitude to a comfortable one,—there we have the acknowledged climate of utmost healthfulness. Going into more torrid lands, the dryness of the air alleviates the heat of the deserts of Arabia, and of Africa, of Peru, and Bolivia, where the temperature rises at times to 110° or even 120° in the shade. I have the assertion of a friend that on a hot day, with the thermometer nearly 100°, he has known the wind on the Arabian Desert to be searchingly cold, when everything was shrivelling up for want of moisture. My attention was called by Dr. J. S. Billings to the following extract of his Report on the Hygiene of the U.S. Army in 1875:—"Description of military posts, Fort Yuma, California. Reports of Asst.-Surgeons Lauderdale and Ross, U.S.A. After describing the locality of the post, near the junction of the Giler and Colorado Rivers, they say:—'The heat increases rapidly from the latter part of May, and in June, July, August, and September may be said to be intense. . . . During the months of April, May, and June no rain falls; then, with the thermometer at 105°, the perspiration is scarcely seen upon the skin, and it becomes dry and hard, and the hair crispy, and furniture falls to pieces, . . . ink dries so rapidly upon the pen that it requires washing off every few minutes. . . . A No. 2. Faber's pencil leaves no more trace on paper than a piece of anthracite, and it is necessary to keep one immersed in water while using one that has been standing in water some time. Newspapers require to be handled with care; if rudely handled they break. Twelve pound boxes of soap, when re-weighed, gave but ten pounds. Hams had lost 12 per cent and rice 2 per cent of original weight. Eggs that have been on hand for a few weeks lose their watery contents by evaporation; the remainder is tough and hard; this has probably led to the story that our hens lay hard boiled eggs.

"'The mercury gained its highest point last summer on the 2nd day of July, when for two hours it stood at 113° in the shade. All metallic bodies were hot to the touch; my watch felt like a hot boiled egg in my pocket. . . .

"'This post, although not the most southerly, is the hottest military post in the United States: the highest temperature recorded on our books since 1850, when the

post was established, is 119°, observed at 2.25 p.m., June 16th, 1859. A temperature of 100° may exist at Fort Yuma for weeks in succession, and there will be no additional cases of sickness in consequence. . . . We have none of the malarial diseases. . . . No ice is formed at any time; 29° has been indicated by a registering thermometer, in January, 1872. The mean temperature, day and night, of January, however, is 57°; that of July is 95°. The average rainfall during four years was a litttle over 2 inches each year.'"

Any who wish to corroborate or question these views as to the healthiness or unhealthiness of dry air, hot or cold, can examine authorities, or investigate or observe for themselves; the conclusions they will reach can be confidently anticipated. But the proof or argument cannot be further extended in this paper, and it must be claimed that there exist good grounds to believe that dry air, *per se*, of whatever temperature it may be found on the surface of the earth, is not unhealthy; that, as regards disease, such air possesses both preventive and curative qualities of great value; and that on the other hand, moist air, such as promotes vegetable growth, is not desirable for breathing, on sanitary grounds. Asserting these views, the question narrows down to—Given a habitat or place of residence, where some degree of moisture and vegetation does thrive for a portion of the year at least, what effect on the system do the variations of moisture produce, from season to season, from day to day, and during such of the seasons as the comfort of inhabitants may call for artificial warmth, from one place to another on the same day?

§. Clothing, houses, and fires are the means by which mankind is enabled to inhabit the face of the earth. It is an artificial existence for an animal whose natural life would otherwise be limited to a small belt of the torrid zone, where the temperature never falls below about 80° nor rises above 100°. As residents of the northern United States we cannot expect to avoid, and do not expect to ameliorate, the vicissitudes of climate *out of doors*. Hot or cold, rainy or dry, with air relatively humid or otherwise, life in all countries means endurance under artificial guards or protections from natural inclemencies. We clothe ourselves by the umbrella on the one hand, or the great coat on the other; open or close our doors; induce cool breezes, or gather around fires, in search of the *comfortable uniform* loss of heat by the system. The efforts to accomplish this end, by means of

change of temperature or relative moisture of the air, are of necessity confined to the cold, or at least cool, season, with infrequent attempts to obtain an artificial degree of cold in extreme hot weather. In moderate weather the vicissitudes of temperature, and of humid condition of the air, are endured with the expression of discomfort and the tacit admission, on all hands, that our great day-by-day variations of the mild season are harmful to the feeble or sickly. But these daily changes of temperature and of hygrometric condition are of small account with those which in the Northern States accompany the season of cold. The change of climate from that which accompanies, during any of the winter months, our warm south winds, to that which accompanies a great north-western wind-wave, which may follow the southerly breeze within twelve hours,—a change from 50° to 60°, with 80 per cent saturation, to even below zero, with an unascertainable dew-point,—such a change is trying in the extreme. The prevalent disease of the land is consumption of the lungs, and these changes are disastrous to those who are suffering from this complaint, and to the healthy these changes are held to be fraught with danger.

§. A very simple and commonplace observation will make the general condition of air in rooms in winter, as regards humidity, the subject of positive demonstration. During the season of winter in our climate, after a continued spell of cold weather, the exhibition of condensed moisture or of frost on the window-panes is very infrequent. The usual provision of glass in windows throughout the northern United States is in single thickness, not double plates; the latter arrangement being decidedly exceptional as a means of preventing transfer and loss of heat at the windows. The temperature of a pane of glass which is interposed between two temperatures of still air—that is, of air devoid of currents, except those generated by the differences of temperatures of the air on either side and the glass—is obviously that of a mean between the said two temperatures of air. With so good a conductor as glass, and with plates as thin as ordinary window-glass, the conductivity of the glass may be assumed to be perfect, and both sides of a pane can be deemed to have the same mean temperature. But the temperature of the air on each side of a pane differs from either that of the external air on the one side, or of the room on the other. On the outside, the layer of cold air in contact with the pane ascends slowly as it is heated, and the vacuity which is formed at the bottom of a

pane is supplied by fresh cold air, so that the layer at this point approximates to the temperature of the outer air closely. On the inside, the layer of warm air in contact with the pane descends as it is cooled, having, perhaps, an approximation to the inside temperature at the top of the pane, but by the time the layer has flown downwards to the bottom of the pane its temperature will have become materially lower than that of air of the room generally. So that while the law of mean temperature of the pane at the bottom of the pane is yet good, the real temperature may be much lower than a simple mean between thermometers hung in the room and out of doors. Beside the supposition of still air, much allowance must be made for the effect of winter winds in accelerating the flow of cold air on the outside of the pane until the outer layer is very nearly of uniform coldness, which favours the greater abstraction of heat from the internal descending current, and cools down the lower part of the pane still further. Curtains, shades, or internal blinds, while they aid in protecting the room from loss of heat, also protect the glass from acquiring the temperature due to the heat of the room. Until with the supposed case of external air at 10° above zero, and a moderate wind out of doors, and of a room warmed to 75° inside, the temperature of the panes near the bottom, or even well up their height, will be much below 42½° as the mean; and 27° to 28° may fairly be taken as giving the real temperature near the bottom. This pane of glass becomes, then, a dew-point thermometer at all times in the winter, ready for indicating the humidity in the air. Except, however, a crowd of people, or some artificial "hydration" carried possibly to momentary excess under the stimulus of the last theoriser, or a very new house, or a damp cellar in an old one, we but rarely see any indication of presence of moisture in our dwellings in cold weather. This simple test will show that our dwellings, although the water-troughs of the hot-air furnaces do supply limited quantities of vapour with admitted comfort, do not, as a rule, have over 30 to 40 per cent of humidity in the air within them.

The entire range of our Atlantic coast is only removed from a region of perpetual spring from 200 to 500 miles; and a south-east wind, from December to April, may bring a vapour-laden air, which, in a few hours, will have changed our frigid winter to a genial spring. The succeeding wave of north-west winds—a great current which the Signal Service has traced up to the Arctic regions—may, with great violence, restore the winter with all its rigour, within

another few hours. Under this change heavily-frosted windows become the rule, and they indicate a dew-point above the temperature of the panes of glass, at once. So dry and arid are these winds, however, that with the continuance of a north-west wind for a single day all traces of window-frosting will have disappeared.

This test of the dew-point by the window is very accurate. As an instance I will quote that on one day in this present month, in Philadelphia, with the thermometer in-doors at 64°, without fire, a fall of temperature to 54° outside, produced immediate condensation on windows, showing 75° to 80° of humidity. Now if a difference of only 10° produces this indication for *humid* air, the want of such an indication with difference of up to 65° must manifestly be a very dry air. By actual trial in well-warmed and ventilated rooms, the writer has found the dew-point far below the freezing-point of water, in rooms where the sensation of dryness, which is held to accompany the heat of the furnace when not supplied with water for evaporation, certainly did not exist.

It is proper, when alluding to the dampness of houses, to advert to one of the most striking differences between England and Western Europe and the northern United States, in the necessities which climate imposes in the relation of humidity in air to the health, in this particular regard. We find all foreign authors speaking in unequivocal language of the great danger of inhabiting a newly-built and consequently imperfectly dried house. One English writer, whose name I cannot recall, but I remember to have been of much eminence, asserted that no house ought to be occupied until a year after completion. Many writers on ventilation join in estimating by figures the quantity of moisture in the new walls, and *demonstrate* the dangers of a residence where the excess of moisture in the air, the want of permeability of the walls, and the increase of conductivity of heat through the damp walls, will have produced such a tomb-like house. While in America we are fully alive to the danger in the house, from air overladen with moisture, as from a damp cellar or location in a damp place or vicinity, and appreciate that rheumatism and consumption, with scarlet fever for the children in winter, when the house is thoroughly warmed, and typhus for adults in mild weather, are the possible, if not probable, penalties for living in such a house; yet the new house, from its inherent dampness, only is considered at least as but objectionable in a small degree. Except that dampness exists from other sources

than that which comes from the walls, our new houses are quite as healthy during the first year, or the first season's occupation, as afterwards. No preliminary drying out is needed as a rule. Our summers or winters are dry enough to take up all the moisture which walls may give, without overlading the air with humidity; although it is noticeable that new houses require more fuel to warm them the first winter than afterwards, as the supply of heat must be sufficient to evaporate an excess of moisture, and the conductivity of the walls is somewhat greater before they are thoroughly dried out. Few dwellings are completed for occupancy at the end of winter or of summer—residence generally begins with the beginning of summer or of winter, and the seasons when the dampness of walls would be dangerous from the existence of a humid air are not those when new houses are generally first occupied.

§. Although not directly related to the subject, I will mention here one curious demonstration of the effect of atmospheric humidity and impurity which is peculiar and common in England. All American readers or observers become aware of the great importance attached by the English public, as a people, as writers, and as a government, to the relative purity of illuminating gas; but it is not generally known, or even asserted, that the cause in England which makes the impurities of gas obvious, and renders them seriously objectionable, is to be found in the air of the room, and not alone or even mainly in the gas itself. The sense of oppression from the burning of gas in dwellings in England is one that can be appreciated only by being felt; any description fails to convey to the mind of the untravelled American the burden on the breathing functions which results from gas burning in a humid and impure air. It is enough to say that throughout England gas-lighting is regarded as only suitable for shops, work-rooms, warehouses, public rooms, and such other places, within or without of doors, as demand light for passengers, rather than for occupants. When halls are lighted by gas, the chandeliers ("gasoliers" is the English word) and bracket lights are not considered to be well arranged unless "ventilated;" in other words, provided with especial means—air-passages or outlets—for removal of the gases of combustion, with the accompaniment of a volume of heated air. The dwellings—dining and drawing rooms, passages, and chambers—of the more wealthy are lighted nearly universally by candles of wax, sperm, or some prepared fatty

substance, either animal, vegetable or mineral, with the occasional or frequent use of the carcel-lamp as a table light. From any English gas-light there arises a current of impurities, which in a brief space of time discolours and coats with black the interior of the ventilating tubes, or, in absence of such protectors, the canopies of glass or metal which are usually supplied for unventilated burners; or when this last protection is wanting, the ceiling, even when several feet above the burner, is quickly marked by a halo of greasy soot, which adheres to it where the ascending current impinges.

The heat emanating from a gas-burner, as compared to that from a number of candles giving the same amount of light, is very nearly the same, about 7 per cent more heat being given out by the candles; but the fourteen to sixteen candles, which represent the single gas-burner, will have been dispersed about a room, or even when grouped in threes or sevens—as usual in sconces, candelabra, or chandeliers—will be spread widely asunder, so that the current proceeding upwards from each separate candle will have become diffused before reaching the ceiling; and, if the candles give out the same impurities (which they do not) as the gas-burners, the obvious impurities which make a mark will not be precipitated to show themselves as spots. Besides this, a room lighted by candles will be considered brilliantly illuminated by three or four candles, where one gas-burner would have been used; so that one-fourth the quantity of light will be made to suffice with candle illumination, to what is requisite for gas lighting. The numerous candles come in proximity to the objects to be lighted, while the gas-burner, with its volume of light and of heat, must be further removed to be tolerable, and there is an actual requirement for more light from the latter than the former source to give the same effect in the room. The other substitute for the gas-burner, the carcel-lamp, is frequently used in England as the centre-piece, singly or in numbers, of the tables in the dining-room, drawing-room, or library. Its illuminating power is about two-thirds that of a gas-burner, and the quantity of heat given out bears very nearly the same relation,—that is, the heat from a carcel-lamp is about two-thirds that from a single gas-burner of fourteen to sixteen candle-power. As the carcel-lamp is movable in a room, and is usually placed low down, the chance of the current of air proceeding from it defacing the ceiling is much less than from a fixed gas-burner, in its usual position of height from the floor.

What may be the exact physical or chemical conditions

accompanying the manifestation of *bad* gas in England might call for a long discussion in reply. It can be said at once that the indications on the ceilings are almost entirely those of atmospheric impurities and condensation of water. Soot, dust, and organic matter in suspension in a saturated humid air—wherein the humidity is the vehicle which carries these substances, and is not free to disperse them from the general saturation—will be and are deposited on the first object of cooling and contact. This view of the case is not a mere guess. The fact that there is a general discoloration of ceilings from gas-burners in England, as contrasted with those of the Continent, where the air is uniformly a little more dry, and where the use of bituminous coal is unknown, so that its particles of carbon dust do not form a part of the common impurities of the air; and also as contrasted with the ceilings in this country; this fact is certain, and from it the connection of pure air with the *cleanliness* of gas burning is made evident. While English gas is much more carefully purified and treated than any in the world, the standard of its excellence is not only the highest, but it is made strictly up to this standard. Yet any sulphurous or sulphuric acids which emanate from gas lights are at once absorbed by the vapour present, and if the atmospheric condition does not facilitate the diffusion of this vapour, these acids are retained in the ascending column to exert their energies on the objects of first contact, and afterwards retained in the room to act generally on any sulphur absorbent, colours or materials, as they do in England and do not in this country. It is probable in our climate, and in that of the Continent, much of the humidity of combustion, and of the deleterious gases, either evolved by or inherent to the illuminating gas, as well as the organic impurities, dust or soot, in the air burnt or heated by the act of burning, is diffused at once into the tenuous vapour of the surrounding air. Not only before passing four or five feet from the burner, so that no condensation takes place on the ceiling, but also so thoroughly diffused as to prevent, in great degree, those chemical actions which prove so objectionable from the burning of the best purified coal gas in England.

§ It has been shown how nearly impracticable it is to procure in winter, with the average temperature of winter, which is 34°, a summer temperature and humidity in our houses. The difficulties of effecting this with 34° for the temperature of the external air are enhanced greatly as the

minimums of cold are reached, and at zero the production of a summer air in a house or place of residence may be claimed to be impossible. If the effect of the changes of out-of-door temperatures and humidities, which can happen with, at the worst, eight to twelve hours of change, and which on the average gives twenty-four to seventy-two hours of interval, is as objectionable, what words can express the effect of the mere passing from a room at summer humid warmth to the open anhydrous air at zero? There are few readers of this paper who have not tried the experiment of leaving some crowded hall, where the closed doors and windows and many breaths had made an approach to the summer condition, and felt the cold air of winter at the bottom of the lungs, as the inactive membrane parted with its unexpected supply of moisture to the anhydrous air. At whatever temperature or moisture condition air be inhaled, it will be exhaled at 90°, and laden with moisture nearly to the point of saturation. Of the heat given out by the lungs, that which proceeds from evaporation is generally largely in excess of what is required to impart heat to the air. Even in the extreme case of breathing air at −40° (or the temperature when mercury freezes, which the writer once observed at Vassalborough, Maine), the heat of evaporation of moisture from the lungs is but a little greater than that for heating the air, being 2·22 units in one case to 2·18 units in the other, per cubic foot of exhaled air. The skin gives out its heat through insensible perspiration, or through heat imparted to air in similar, if not the same, proportion.

The establishment of a régime of evaporation from the lungs or skin—of a constancy of secretions—appears to be more essential than the establishment of a uniform temperature, either of the air of respiration or of contact with the person. The stability of the moisture condition, whether in the external air from time to time, or between inside and outside of the room, is what is desirable for health; and this stability, from inside to outside of room, is what we must maintain if we are to live in active, healthful life in our climate. The transfer of heat through the skin or membranes is merely conductive, not involving organic action; while the supply of moisture incident to the maintaining of evaporation brings into service vessels, ducts, or pores, whose healthful action depends in great measure on the regularity and continuity of the said service. This hypothesis will explain at once the healthfulness of the climate of Florida or that of Minnesota in cases of pulmonary disease, and in other parts of the

country will account for the prevalence of colds and coughs or the occurrence of rheumatic affections. The diseases of the changeable climate and water-laden winds of our colder Eastern States are bronchial and pulmonary; and, without desiring to entrench on the province of the medical profession, whose known duty at all times it has been to find the cause of disease) it may be safe to attribute them, to a great extent, to the effect of alternate dry and damp air on the evaporating surface of the lungs when the skin has the protection of clothing to keep it warm. The disease of the warmer mountain-sides of our Middle States are rheumatic, and may be attributed to the same cause operating, by warm currents of air, on the less unprotected skin.

Beside this view, it can be averred that, for the resident or native of any country, there will have established as a habit a connection of moisture of air relative to its temperature which is national, so to speak, in which the *variations* of humidity and heat are accepted as a general average. Thus, the American in England is chilled to great discomfort by the low temperature endured by Englishmen, whose systematic evaporation provides for small loss of heat; while the Englishman in America finds a suffocating heat in the dwelling of the American, from the fact that his lungs and skin do not afford the moisture requisite for evaporation and consequent dispersion of heat. A long residence, often two or more years, is needed before the system of either is adapted to the novel condition.

§. Although somewhat late in the course of this argument, and perhaps somewhat elementary as information, it may be well to state the physical laws of the relation of moisture to air. The property of water is to possess in contact with itself, at any and all temperatures, from the boiling-point downwards, an atmosphere of vapour, which vapour has an elastic force, or exerts a pressure bearing some definite relation to the sensible temperature of the water and of itself. The English measures of this elastic force are generally expressed in inches of height of a mercury column, in the same way as shown by an English barometer, which of course applies to any unit of surface, and may be transformed to pressure in pounds per square inch or square foot, by a similar process to what we use for atmospheric pressures. According to Regnault (as quoted in Guyot's tables), some of the elastic forces are as follows:—

Degrees Fahrenheit.

−30	−20	−10	0	10	20	32	40	50	60	70	80	90	100	212
0·0092	0·0163	0·0270	0·0434	0·0684	0·1078	0·1811	0·2476	0·3608	0·5179	0·7329	1·0232	1·4097	1·9179	2·9922

Inches of Mercury Column.

From which it will be seen that the elastic force falls off rapidly with the temperature. At any given temperature vapour, possessing the above value of elastic force, exists in the atmosphere as a part of its volume, *whenever there is water present to supply it*, and such an atmosphere is said to be saturated. When there is not sufficient water at hand to supply the vapour due to the temperature, what there is of vapour in the air is slightly superheated, as it accepts the temperature of the air and not that of its tension, but this effect is so small that it may be neglected. The air is then said to be dry; the usual way of estimating such dryness is by naming the percentage of vapour present to that which would fully saturate the air at a given temperature. Dry air seeks moisture from any source, and the difference of elastic forces, between that of the vapour in the air at any time, and that of saturation of the same air, represents the *avidity* for moisture of such dry air as a species of partial vacuum.

Now, the quantity of moisture as vapour accompanying a given quantity of air is neither increased nor diminished in the same air by heating or cooling it (of course, in the latter case to the temperature when the air is saturated, below which point the moisture condenses). Hence it does not matter, so far as moisture is concerned, at what degree of heat we introduce the air for warming a room; if only the final temperature of the room be 70° or 80°, then the drying effect of the air of that room upon the persons occupying it, or its furniture, or its materials of construction, is that due to air of 70° or 80°, which air shall contain only the normal moisture of supply. In other words, our hot air furnaces which supply air at 150° to 200° do not (except, perhaps, close to the mouths of supply, before the heat is distributed) dry up wood-work or absorb any more moisture from the persons occupying a room any more than do currents at 80° or 90°, provided the general temperature of the room is the same in both cases. But a yet more important truism can be stated, which is that the drying effect of air of a ventilated room at 70° or 80° which has received no increase of humidity in the hot air of ventilation from out of doors—air that has been warmed artificially from zero, let us say—the drying effect of this heated air upon a person occupying it is very nearly the

same as if he or she were to maintain the same temperature through active exercise and warm clothing out of doors. The exception expressed by "very nearly" relates to the clad portion of the body—the obstacle presented by the clothing to the free diffusion of aqueous vapour is more effective between the cold air which is but little warmed to demand moisture, and the skin which will give it out if the vacuum demands it, than between the arid air of the room, which takes up every particle of moisture as it transpires through the clothing. As regards loss of moisture from the throat or lungs, however, there is absolutely no difference in breathing the air of the supposed room and that which is then found out of doors, although the one be at 70° to 80° and the other at zero; reiterating the former statement:—"In either case the exhaled breath is at 90° as it passes out from the nostrils or lips, and is saturated or nearly saturated with moisture." No one ventures to assert that it is unhealthy, as an act of breathing, for the human race to breathe freely the coldest dry air of winter, because of the supplying of moisture to the anhydrous air. There is an evident fallacy in the assumption that it can be healthy to check instantly that copious secretion which has been supplying moisture to the fresh cold air of zero, by going into a room of summer hygrometric condition, or to demand such an effort of the tissue of the lungs suddenly by leaving such a room for the outer air. Fortunately, Nature is more lenient than the theorists, and we do not get 70° to 80° with 70 per cent. of saturation in the most unventilated or uncomfortably heated houses, and with all efforts to the contrary, even 40 per cent. is of unusual attainment when the external air has a temperature of zero.

It must be admitted, however, that some small degree of hydration is a necessity for comfort, and, with comfort for a demand, some reason may be found to establish the healthfulness of the small supply. It is certain from all experience that from 5 to 10 per cent. of moisture can be added to air after it is heated, certainly with much relief, especially to the eyes, with apparently little harm, although such addition may make the occupant of a heated room a little delicate as to out-door exposure. Moisture may to some small extent be abstracted by the means of heating, especially when the heating is by stoves or hot-air furnaces; at all events, the presence of a sheet or surface of water over which the heated air is allowed to pass is now a recognised means of supplying a small quantity of aqueous vapour to air of ventilation. But the quantity supplied in this way is

very small in comparison with what is needed for complete "hydration," or even for what can be denominated "hydration" at all in the sense of a summer condition. From an estimate based on several winters' experience, a vaporisation of water which supplied a half grain of vapour per cubic foot of air introduced, when an increment of four to six grains for the same volume of air would be requisite to get the summer condition of humidity corresponding to the internal temperature, has proved sufficient to give a sensibly pleasant air, while the absence of this supply was at once perceptible in the house. A low pressure hot-water apparatus, whose temperature never reached the boiling-point, and rarely exceeded 180°, giving heat to a large volume of fresh air, which, at the mouths of supply to the rooms, was not much above 90° at any time, was the source of heat.

§. It is very difficult to find any hypothesis which will account for this requirement of a small supply of vapour with heated air when we admit, or can demonstrate, that the sufficient quantity to the senses is so far below what is needed for hydration, and so independent from the moisture condition of the air; for nearly the same small quantity of vaporisation seems desirable in air heated from any temperature. The explanation of the offensiveness of heated air currents has been sought with much diligence, and, at times, causes have been assigned with much positiveness. One of the earliest of these explanations (but one which yet finds supporters) was found in the substitution of transferred or convected heats by currents of air for the radiant heat of fire. Open fires were to be restored as the means of securing pleasant air. The healthfulness and comfort of our ancestors were to come back to their degenerate children. Gathered around a blazing fire, roasted on one side and frozen on the other; restricted to one fire in the house, as all the others *would* smoke; the chamber-heating apparatus reduced to the warming-pan; victims of rheumatism, sciatica, tic-doloreux, and ague—the diseases of fifty years ago—the good old times were to come back. Alas! there were obstinate innovators, and the world would not be convinced of the advantages of radiant heat as the sole means of warming.

This point being established, at a later time, surfaces at high temperatures for imparting heat to the air of a room, either by ventilating currents or direct heating, including all fire-heated surfaces, together with steam-heated ones, above the boiling-point, or 212°, were utterly condemned.

Somebody discovered burnt atoms of dust in heated air, and its destructive, pernicious effect on the health was at once apparent! But the comfort of the community some way overruled the theory, and hot-air furnaces, with admitted deficiencies in quality of air, met greater favour than ever. It is allowed generally that the expensive steam-heating apparatus is at once the more pleasant, controllable, and durable; and that the yet more expensive hot-water apparatus, with its great volumes of low temperature currents of air, is the best of all means of heating. The cost in fuel by these several apparatuses becomes nearly the same, for equal effects, in warming of houses.

The next demonstration was the chemical one. An occult effect is most conclusively, if not convincingly, explained by an occult cause. Ozone is a favourable object to carry a theory, and it really is possible, if we knew anything definite and certain about the origin or the effect of ozone; some relation of this phenomenon of the requiring the evaporation of a small quantity of water, when heating air, might be traced. But no blinder pathway in science was ever opened than the ozone one. After this came Deville and Troost's discovery of the permeability of some metals, when heated at, or nearly at, red heat, to some of the gases. In the language of one of the most prominent writers on ventilation, this "explains the very injurious and even poisonous effects produced by the use of stoves in the rooms of a dwelling!"

The last resort of the unreasoning theoriser in physics is always to electricity, and efforts have not been wanting to show that either the presence or the absence of electricity, in some form or condition, ought to have something to do with the discomfort arising from heated air. The only answer to this hypothesis is, that heated air is equally oppressive in entire absence of water supply, whether highly electrical or otherwise; our vicissitudes of climate and of humidity enabling a test of electric condition in extreme cases. There are times in any winter, in the Northern States, when it is possible to gather enough electricity, by walking over a carpet, to make the spark from the finger which will light a gas-light. The quantity of water demanded at such times by a heating apparatus is no greater than at other times. There is not the least positive proof of relation of electricity to the healthfulness of air. Altogether, the whole resolves itself to the reiteration of the bare fact that it is comfortable to evaporate a small quantity of water in heated rooms, and that it can be done without

marked injury to the occupants or to visitors. The quantity itself seems to be almost constant for all temperatures or hygromations of the air, and to be a slight addition only to the moisture in the normal air out of doors at any time.

§. The effect of draughts or currents of air upon any person exposed to them is materially modified by the hygrometric condition. In still air in winter the comfortable temperature of a room in general hygrometric condition has been stated at 70°, but a current of air upon the person at this temperature is uncomfortably cold from the rapid abstraction of heat, while in summer the same temperature, accompanied by the summer dew-point, will be warm enough to demand light clothing, and a current of the same velocity will not be objectionable; in other words, draughts which cannot be tolerated in our houses in winter become pleasant breezes in summer. Not only the speed or rate of velocity of the current of air is to be taken into account, but its avidity to take up moisture from the skin as indicated by its dew-point. So long as the hygrometric condition of the air is such that will absorb moisture below 98°, a blast of it at some rate of current will be a cool one.

One fans himself in our climate, when the thermometer stands at 100°, with a sensation of relief. This feeling of cold, from air of high temperature, when in motion, proceeds from the rapid removal of the stratum of warm and nearly saturated air in contact with the person, and its replacement by fresh air, which is not only cooler, but which has not yet become saturated or charged with moisture by contact with a moist surface like that of the skin. In no one of the changes in the three forms of matter—solid, liquid, and gaseous—is there so much heat taken up as in the change from a liquid to a gaseous (or vaporous) form, and in no other body or substance is so much heat absorbed or become latent as in the formation of steam from water, or, in other words, in the process of evaporation; and the quantity of heat taken up by the moisture which a dry air abstracts from the skin is so great that the mere differences of temperature of the air from that of the skin may almost be neglected in the statement; and it is very nearly correct to assert that the cool sensation from a breeze in summer proceeds entirely from the evaporation of moisture thereby induced.

Upon this basis it may be noticed that a current of saturated air at 100° would neither remove heat by its contact nor by induced evaporation, and consequently would be

incapable of producing a cooling effect, while as the temperature or the dew-point should fall the current would become a pleasant one. With a high temperature and dry air the cooling effect of a current of air (even at 100°) may be very pleasant in the sensation, but will be attended with sun-burning (even without exposure to the sun), and blisters will be produced by the excessive deprivation of moisture from the cuticle or surface of the skin. With 80° of temperature and a high dew-point a strong breeze is not unpleasant, nor likely to be injurious after the person shall have acquired some *accustomed* habit of body to endure it; but at 70° and a low dew-point, which is the only possible condition of heated air in mid-winter, the annoyance of a current of even 5 feet per second and its unhealthiness are positive facts.

§. There remain to be considered two more relations of moisture in air to health and comfort. First, the effect of evaporation of water by the air itself in summer, in reducing the temperature to one of comfort; and secondly, the effect on the moisture condition of the air of summer, when it is attempted to cool air by artificial means.

The cooling of air, by spontaneous evaporation from extended surfaces, is of frequent practice in hot countries by the wealthy inhabitants near the banks of rivers where the water-supply is abundant. The condition of the air which makes it practicable is one of great heat, and of a relatively low dew-point; and the summers of Egypt and of parts of India, especially of Bengal, give opportunity to employ this method of cooling air. If it is accepted that the temperature of the air in the shade, in the localities referred to, will sometimes run from 85° to 105° for many consecutive hours, accompanied with, say, 50 per cent of moisture for the 85° of temperature, or, say, 30 per cent for the 105°, then the evaporation of moisture from wet surface can be relied upon to produce a comparatively comfortable atmosphere. Air at 85°, with 50 per cent of moisture, has quite exactly the same quantity of moisture, per given volume of air, as that at 70° and 70 per cent of moisture. So that if it could be cooled *without* adding moisture at all it would then reach the point of comfort for the clothed inhabitant of the temperate zone. If the dew-point of 85° is brought up to 80 per cent, or above, the air becomes intolerably sultry, and at 90 per cent quite suffocating; so that the greatest addition of moisture practicable to the supposed air of 85° and 50 per cent dew-point may be taken

XUM

as 10 per cent, or 1¼ grains to the cubic foot. The resulting figures which the latent heat demanded for the evaporation of these 1¼ grains of moisture into the air supposed is 74½°, and 82 per cent of humidity. How far this condition of the air may be more comfortable than the original one of 85° and 50 per cent of humidity is questionable; but it is apparent that the limit of possible cooling of air of 85°, by evaporation of moisture, is found when its humidity is not to exceed 50 per cent of saturation. A similar computation applied to air of 105° (in which air there is a little more moisture than in that supposed at 85°) gives, for the addition of 1¼ grains of moisture to the cubic foot, air of 94½° temperature and 48 per cent of humidity—an atmosphere which *may* be, in some degree, more comfortable to a person at rest than the normal one. It is evident that, for efficiency in cooling air of 105° by evaporation from moistened surfaces, the humidity of the air to be cooled should be less than 30 per cent. As to ultimate healthfulness of the moistened air, it *seems* to be unquestionable that the supply of moisture ought to promote disease.

We have, however, in each year in our country, a few days or parts of days (perhaps, in the Southern Middle States, ten to twenty days in different years) when the range of thermometer and the dew-point make it feasible to adopt this means of reducing the apparent heat of the air. The attempt has been frequently made, with provoking failure to the projectors. Its success depends upon not only the exact condition of relative humidity and temperature, but also on the proportion of surface of evaporation to the quantity of air to be supplied. The Indian ratio is one or two persons to 6 to 8 square yards of wet surface. But the most provoking cause of failure has been, that while there are ten to twenty hot dry days in any year, there are also twenty to thirty hot damp ones, and the application of the *cooling* apparel to the hot air on these days has produced such an effect of sultriness that the whole has met with instant condemnation.

The last relation of moisture to air to be considered at this time is that which proceeds from the effort to cool air artificially. The fallacies of the attempts to effect this purpose can be made very apparent. Even the smallest decrement of heat is obtained only at great cost. The *quantity* of cooling of air in summer is, to be sure, only about one-fourth that of heating in winter. Taking the ideal temperature of 70°, there may be 15° to come off in summer as generally as 60° to be added in winter; and sup-

posing iced water to be the cooling medium, and steam of low pressure to be the heating one, the relation of difference of temperature of the air to be cooled or heated to that of the iced water or steam is such that about the same extent of surface would be required in either case to transfer the heat. But a pound of coal produces, in ordinary steam-boilers, quite 9000 units of heat on the average, while a pound of ice (in cooling to 60°, let us say) will produce only 170°, so that about 54 lbs. of ice would be demanded to effect the transfer of an equal quantity of heat, to what would be effected by 1 lb. of coal; or, accepting the one-fourth cooling effect, then 13½ lbs. of ice would be demanded for the cooling of air in summer against each pound of coal required for warming in winter. Unfortunately for the proposition for cooling *air* in summer, even this statement is too favourable, for the requirement will be found that the air must not only be cooled, but must be divested of a portion of its moisture, unless the cooled air is deemed satisfactory in the form of a cloud of vapour. Air at 85° and 70 per cent of humidity must be taken to be cooled to 70° and 70 per cent of humidity, and one and one-fourth times as much cold will be demanded to condense the vapour 2·3 grains per cubic foot as that which is requisite to cool the air the 15° supposed. This leaves the ratio of ice needed to obtain a spring condition for the air on a hot day in summer to be that of 30 to 1 of coal usually demanded to heat air on a cold day in winter, or assuming that the air on such a day is so dry that no moisture should be removed, about 15 to 1. Our ideas of the necessity of civilised wants, as compared with civilised luxuries, scarcely yet reach high enough to warrant the expenditure of money to cool air under the circumstances stated.

In view of the great cost of cooling air by ice, it has been proposed to cool it by mechanical means on a large scale. Air, if compressed, becomes sensibly hotter. In fact the compression can be carried to the extent that the heat will ignite tinder, as the cigar-lighters of twenty or thirty years since bear witness. And it has been proposed to use large air-pumps which shall compress the air until its temperature is elevated sufficiently for it to give off heat to surfaces cooled by currents of water, at such temperature as water is to be had from streams or aqueducts in summer. This compressed air, when deprived of a portion of its heat, is then allowed to expand, and the result is a cooler air. This process has, in reality, much merit, and it is probable that the cost of producing cold in this way compares favourably

with the use of ice; in fact it has been shown that ice itself may be manufactured with profit by this process in some localities, where transportation enhances the price of ice largely; but it is preposterously expensive in apparatus and in cost of working as a means of cooling air in the great quantities demanded for ventilation, and the humidity of the cooled air would still be objectionable. The comparatively high temperature of the surface for cooling the air would fail to be very efficient in condensing the vapour thoroughly. In both the methods of cooling air, whether by ice or by water (of which great quantities would be needed), the cooling surface must be copper, brass, or tin, as the rusting of iron, when exposed to condensing vapour, is extremely rapid.

The most probable result of cooled air would be a thunder-cloud in miniature. The atmosphere on one of our hottest and most sultry days of summer is on the verge of a tempest. Cooling of air of 85° to 90°, to the extent of 20° or 30°, produces a dense mist of super-saturated cooled air. The equilibrium of the atmosphere on a still, clear summer day, when every growing thing on the surface of the ground is supplying moisture, and the radiation of the ground itself is supplying heat to increase the relative levity of the strata of air next the ground—the equilibrium of such an atmosphere is very unstable. Let an upward flow be established anywhere, and the air will rush in all directions along the surface to supply the partial vacuum. The ascending column, as it reaches the region of lower barometrical pressure, will expand, become sensibly cooler, and in a short 5 to 8 miles of height the region of frost and ice will be reached, and hailstones will be returned from the condensation of the transparent vapour which had existed in the air when it left the surface of the ground. The writer once saw in a little ball-room, on a Christmas Eve, a miniature snow-storm deposit a little bank of snow, from the opening of windows to air the room when the dancers had retired, the night being a clear moonlight one, with the thermometer a little above zero.

The difficulty of absence of moisture in air that is heated in winter is a matter to be disposed of with some happiness, by asserting it is not wanted, but the objection of presence of moisture in cooled air can be only overcome by not cooling the air. It does not seem that the successful cooling of our summer air, so as to produce a comfortable or healthful condition at spring temperatures, has any probability of accomplishment.

Quoting, as an appropriate final remark, the words with which I concluded another paper having an especial application to a system of ventilation:—"I will not follow further the proposition to change the seasons into a perpetual spring-time—practical ventilation is the supply in dwellings of an abundance of fresh air at the several seasons, warmed to the temperature of comfort in winter, and supplied in quantity to the volume of comfort (as near as possible of the quality and condition of out of doors in the shade) in summer. The truth is, all our heating and ventilating appliances are the compromise of condition—a truth extending beyond all mechanical operations to the phenomena of Nature itself."

VI. ON SPACE OF FOUR DIMENSIONS.*

By J. C. Friedrich Zöllner,
Professor of Physical Astronomy in the University of Leipzig.

In the first treatise the author shows that both Newton and Faraday were advocates of the theory of direct action at a distance through a vacuum, in opposition to the views of many modern scientific men. In the last

* Wissenschaftliche Abhandlungen von Joh. Carl Friedrich Zöllner, Professor der Astrophysik an der Universität zu Leipzig. Erster Band. Leipzig: L. Staackmann, 1878. (With portraits and facsimiles of Newton, Kant, and Faraday. 8vo., 732 pages.)

CONTENTS.

1. On Action at a Distance.
2. Emil du Bois Reymond and the Limits of Natural Knowledge.
3. Newton's Law of Gravitation and its Derivation from the Static Effects of Electricity.
4. The Laws of Friction, and their Deduction from the Dynamic Effects of Electricity.
5. As to the Existence of Moving Electric Particles in *All* Bodies.
6. Adhesion and Cohesion as deducted from the Dynamic Forces of Electricity.
7, 8, 9. The Mechanical, the Magnetical, and the Electrical Effects of Light and of Radiant Heat.
10. Radiometrical Researches.
11, 12. On the Theory of Electric Emission and its Cosmical Application.
13. Thomson's Demons and the Phantoms of Plato.

treatise, which is of the highest interest, the author describes experiments which he made in Leipzig, in December, 1877, with Mr. Henry Slade, the American. These experiments were only the practical application of Gauss's and Kant's theory of space, which these two eminent men imagined might contain more than three dimensions. The author will try to give to the readers of the "Quarterly Journal of Science" an idea of this theory, though he must of course refer to the work itself for a more ample explanation of it.

In accordance with Kant, Schopenhauer, and Helmholtz, the author regards the application of the law of causality as a function of the human intellect given to man *a priori* —*i.e.*, before all experience. The totality of all empirical experience is communicated to the intellect by the senses—*i.e.*, by organs which communicate to the mind all the sensual impressions which are received at the *surface* of our bodies. These impressions are a reality to us, and their sphere is two-dimensional, acting not in our body, but only on its *surface*.

We have only attained the conception of a world of objects with three dimensions by an intellectual process. What circumstances, we may ask, have compelled our intellect to come to this result? If a child contemplates its hand, it is conscious of its existence in a double manner—in the first place by its tangibility, in the second by its image on the retina of the eye. By repeated groping about and touching, the child knows by experience that his hand retains the same form and extension through all the variations of distance and positions under which it is observed; notwithstanding that the form and extension of the image on the retina constantly change with the different position and distance of the hand in respect to the eye. The problem is thus set to the child's understanding, How to reconcile to its comprehension the apparently contradictory facts of the *invariableness* of the object, together with the *variableness* of its appearance. This is only possible within space of three dimensions, in which, owing to perspective distortions and changes, these variations of projection can be reconciled with the constancy of the form of a body.

So, likewise, in the stereoscope, the representation of the corporeality—*i.e.*, of the third dimension—springs up in our mind when the task is presented to our intellect to refer at once two different plain pictures, without contradiction, to one single object.

Consequently our contemplation of a three-dimensioned

space has been developed by means of the law of causality, which has been implanted in us *a priori*, and we have come to the idea of the third dimension in order to overcome the apparent inconsistency of facts, the existence of which experience daily convinces us.

The moment we observe in three-dimensioned space contradictory facts,—*i.e.*, facts which would force us to ascribe to a body two attributes or qualities which hitherto we thought could not exist together,—the moment, I say, in which we should observe such contradictory facts in a three-dimensioned body, our reason would at once be forced to reconcile these contradictions.

There would be such a contradiction, for example, if we were to ascribe to one and the same object at once mutability and immutability, the most universal attribute of a body being the quantity of its ponderable matter. In conformity with our present experience we consider this attribute as unalterable. As soon, however, as phenomena occur which prove it to be alterable, we shall be obliged to generalise our representation of the ideality of a body so as to bring the observed change in the quantity of its matter in accordance with its hitherto-imagined unchangeableness.

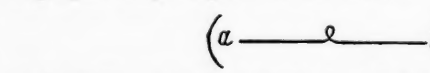
On page 235 of his book the author quotes the celebrated mathematician Riemann, who says in his work "Concerning the Hypotheses upon which Geometry is Founded:"—

> "The explanation of these facts can only be found by starting from the actual theories of the appearance of all phenomena which are confirmed by experience, and of which, as they now are, Newton has laid the foundation. Urged forward by facts, which we cannot explain through our hitherto-conceived theories, we slowly remodel our conceptions. If phenomena occur which, according to our conception, were to be expected with probability, our theories are confirmed, and our confidence in them is founded upon this confirmation by experience. If, however, something occurs which we do not expect, which according to our theory was improbable or impossible, the task is imposed on us to remodel our theory, in order to make the observed facts cease to be in contradiction with our improved theory. The completion of our system of ideas forms the explanation of the unexpected observation. Our conception of nature by this process grows slowly to be more complete and more just, at the same time it retreats more and more beneath the surface of appearances."

I now proceed to apply the higher conception of space to the theory of twisting a perfectly flexible cord. Let us consider such a cord to be represented by *a b*, showing us, when stretched, a development of space in *one* dimension,—

(a————b).

If the cord is bent so that during this action its parts always remain in the same plane, a development of space in *two* dimensions will be required for this operation. The following figure may be given to the cord,—



and all its parts, if conceived of infinite thinness, may be considered as lying in the same plane, *i.e.*, in a development of space in two dimensions. If the flexible cord, without being broken, has to be brought back into the former figure of a straight line, in such a manner that during this operation all its parts remain in the same plane, this can only be effected by describing with one end of the cord a circle of 360°.

For beings with only *two*-dimensional perceptions these operations with the cord would correspond to what we, with our *three*-dimensional perception, call a knot in the cord. Now if a being, limited on account of its bodily organisation to the conception of only *two* dimensions of space, possessed, nevertheless, the ability of executing by his will operations with this cord which are only possible in the space of *three* dimensions, such a being would be able to undo this two-dimensional knot in a much simpler way. Merely the turning over of part of the cord would be required, so that after the operation, when all parts again lie in the same plane, the cord would have passed through the following positions:—

By the same operations, but in an inverted sense, such a being would be able again to form the knot without needing that circumstantial process, during which all parts of the thread have to remain in the *two*-dimensional space of perception.

If this consideration, by way of analogy, is transferred to a knot in space of *three* dimensions, it will easily be seen that the tying as well as the untying of such a knot can only be effected by operations, during which the parts of the

cord describe a line of *double* curvature, as shown by this figure :—

We three-dimensional beings can only tie or untie such a knot by moving one end of the cord through 360° in a plane which is *inclined* towards that other plane containing the two-dimensional part of the knot. But if there were beings among us who were able to produce by their will four-dimensional movements of material substances, they could tie and untie such knots in a much simpler manner by an operation analogous to that described in relation to a two-dimensional knot.

It is by no means necessary—nay, not even *probable*—that such beings should have a contemplative consciousness of these actions of their will. For all our conceptions in relation to the movements of our limbs, and to those produced by their means in other bodies, have been acquired by us solely by way of *experience*. Having observed from childhood that a voluntary movement of our limbs is always connected with a corresponding change in our visional impressions, accompanying the action of our will, it is only in this way that we are now able to connect the movements of our body or of other objects with a corresponding conception of such motion.

Berkeley demonstrated this truth in the year 1709 in his "Essay Towards a New Theory of Vision" and in his "Principles of Human Knowledge." In the last-mentioned treatise he remarks, on the relation of our visional perceptions to the sensations of touch :—

> "So that in strict truth the ideas of sight, when we apprehend by them distance, and things placed at a distance, do not suggest or mark out to us things actually existing at a distance, but only admonish us what ideas of touch will be imprinted in our minds at such and such distance of time, and in consequence of such or such actions."—Berkeley, *Principles of Human Knowledge*, (Fraser's Edition, vol. i., p. 177.)

Lichtenberg, in 1799, expresses himself in like manner when he says :—

> "To perceive something *outside* ourselves is a contradiction; we perceive only *within* us; that which we perceive is merely a modification of ourselves, therefore, *within* us. Because these modifications are independent of ourselves, we seek their cause in *other* things that are outside, and say, there are *things beyond us*.

> We ought to say, '*præter nos*;' but for '*præter*,' we substitute the preposition '*extra*,' which is something quite different, *i.e.*, we *imagine* these things in the space *outside* ourselves. This evidently is *not* perception, but it seems to be something firmly interwoven with the nature of our sensual perceptive powers; it is the form under which that conception of the '*præter nos*' is given to us—the form of the sensual."

The want of these conceptions would necessarily be felt by us, if in some individuals, and these only occasionally: the will should be capable of producing physical movements, for whose geometro-mathematical definition a four-dimensional system of co-ordinates is necessary.

To my knowledge Gauss was the first to direct from the point of view of the "Geometria Situs," his attention to the theory of the twistings of flexible cords. In his manuscripts left behind (Gauss's Werke, vol. v., p. 605), we find the following remarks:—

> "Of the '*Geometria Situs* which Leibnitz foresaw, and on which to throw a feeble glance was allowed only to a few mathematicians (Euler and Vandermonde), we, after a lapse of 150 years, know and possess hardly more than nothing. One of the principal problems on the boundary of the *Geometria Situs* and the *Geometria Magnitudinis* will be to calculate the number of the twistings of two closed and endless cords."

In my first treatise, "On Action at a Distance," I have discussed in detail the truth, first discovered by Kant, later by Gauss and the representatives of the anti-Euclidian geometry, viz., that our present conception of space, familiar to us by habit, has been derived from experience, *i.e.*, from empirical facts by means of the causal principle existing *à priori* in our intellect. This in particular is to be said of the three dimensions of our present conception of space. If from our childhood phenomena had been of daily occurrence, requiring a space of four or more dimensions for an explanation which should be free from contradiction, *i.e.*, conformable to reason, we should be able to form a conception of space of four or more dimensions. It follows that the *real* existence of a four-dimensional space can only be decided by *experience*, *i.e.*, by observation of *facts*.

A great step has been made by acknowledging that the *possibility* of a four-dimensional development of space can be understood by our intellect, although, on account of reasons previously given, no corresponding image of it can be conceived by the mind. (Dass die moeglichkeit

eines vierdimensionalen Raumgebietes *begrifflich* ohne Widerspruch *denkbar*, wenn auch nicht *anschaulich vorstellbar* ist.)

But Kant advances one step further. From the *logically* recognised *possibility* of the existence of space having more than three dimensions, he infers their "very probably *real* existence when he verbally remarks:—

"If it is *possible* that there be developments of *other* dimensions in space, it is also very *probable* that God has somewhere produced them. For His works have all the grandeur and variety that can possibly be comprised."

"In the foregoing I have shown that several worlds, taken in a metaphysical sense, *might* exist together, but, at the same time, here is the *condition*, which, according to my belief, is the only one which makes it probable that several such worlds *really exist*."—(Kant's Works, vol. v., p. 25.)

I may further cite the following observations of Kant:—

"I confess I am much inclined to assert the existence of immaterial beings in this world, and to class my soul itself in the category of these beings."

"We can imagine the possibility of the existence of immaterial beings without the fear of being refuted, though, at the same time, without the hope of being able to demonstrate their existence by reason. Such spiritual beings would exist in space, and the latter notwithstanding would remain penetrable for material beings, because their presence would imply an acting power in space, but not a *filling* of it, *i.e.*, a resistance causing solidity."

"It is, therefore, as good as demonstrated, or it could easily be proved, if we were to enter into it at some length; or, better still, *it will be proved in the future—I do not know where and when—that also in this life the human soul stands in an indissoluble communion with all the immaterial beings of the spiritual world; that it produces effects in them, and in exchange receives impressions from them, without, however, becoming humanly conscious of them, so long as all stands well.*"

"It would be a blessing if such a systematic constitution of the spiritual world, as conceived by us, had not merely to be inferred from the—too hypothetical—conception of the spiritual nature generally, but would be inferred, or at least conjectured, as probable from some *real and generally acknowledged observation*."—(Kant's Works, vol. vii., p. 32.)

XUM

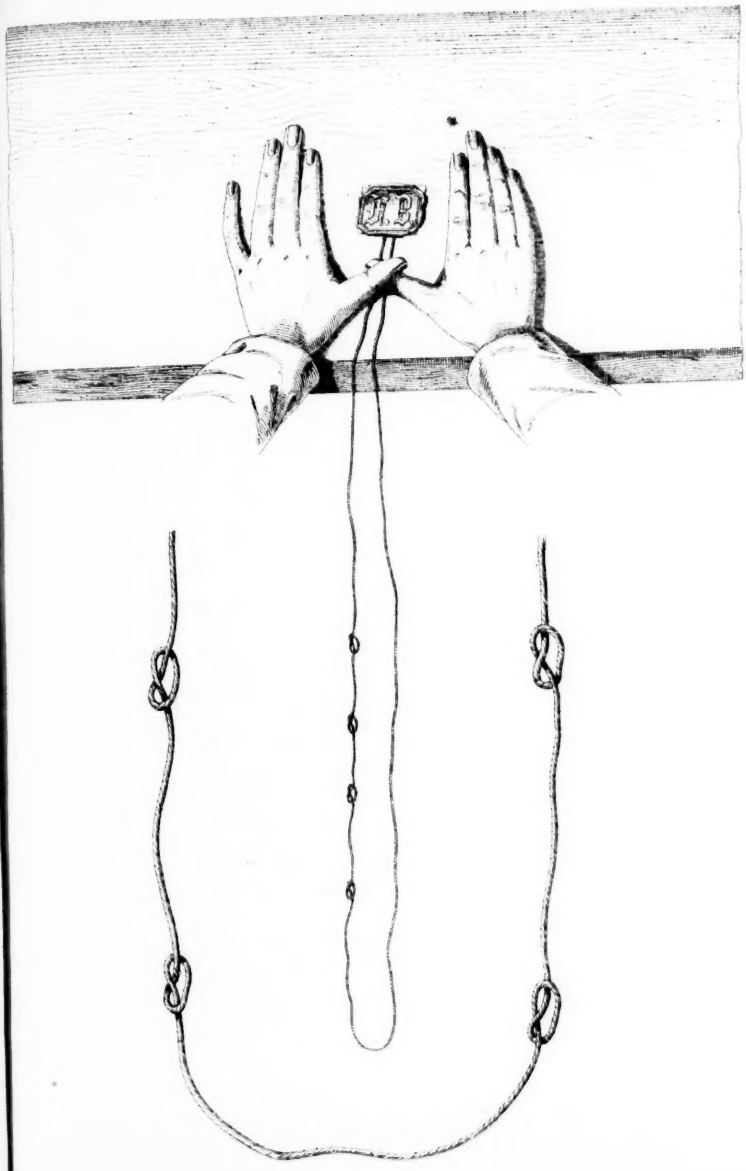
I have already in the above-cited treatise discussed some physical phenomena, which must be possible for such four-dimensional beings, provided that under certain circumstances they are enabled to produce effects in the real material world that would be visible, *i.e.*, conceivable to us three-dimensional beings. As one of these effects, I discussed at some length the knotting of a single endless cord. If a single cord has its ends tied together and sealed, an intelligent being, having the power voluntarily to produce on this cord four-dimensional bendings and movements, must be able, *without* loosening the seal, to tie one or more knots in this endless cord.

Now, this experiment has been successfully made within the space of a few minutes in Leipzig, on the 17th of December, 1877, at 11 o'clock a.m., in the presence of Mr. Henry Slade, the American. The accompanying plate shows the strong cord with the four knots* in it, as well as the position of my hands, to which Mr. Slade's left hand and that of another gentleman were joined. While the seal always remained in our sight on the table, the unknotted cord was firmly pressed by my two thumbs against the table's surface, and the remainder of the cord hung down in my lap. I had desired the tying of only *one* knot, yet the *four* knots—minutely represented on the drawing—were formed, after a few minutes, in the cord.

The hempen cord had a thickness of about 1 millim.; it was strong and new, having been bought by myself. Its single length, before the tying of the knots, was about 148 centimetres; the length therefore of the doubled string, the ends having been joined, about 74 centims. The ends were tied together in an ordinary knot, and then—protruding from the knot by about 1·5 centims.—were laid on a piece of paper and sealed to the same with ordinary sealing-wax, so that the knot just remained visible at the border of the seal. The paper round the seal was then cut off, as shown in the illustration.

The above-described sealing of two such strings, with *my own* seal, was effected *by myself* in my apartments, on the evening of December 16th, 1877, at 9 o'clock, under the eyes of several of my friends and colleagues, and *not* in the presence of Mr. Slade. Two other strings of the same quality and dimensions were sealed by Wilhelm Weber with *his* seal, and in his own rooms, on the morning of the 17th

* In the enlarged drawings the knots have been represented by mistake symmetrical; they were tied on one side, in accordance with the small figure of the cord.


J.B.

of December, at 10.30 a.m. With these four cords I went to the neighbouring dwelling of one of my friends, who had offered to Mr. Henry Slade the hospitalities of his house, so as to place him exclusively at my own and my friend's disposition, and for the time withdrawing him from the public. The *séance* in question took place in my friends' sitting-room immediately after my arrival. I myself selected one of the four sealed cords, and, in order never to lose sight of it before we sat down at the table, I hung it around my neck—the seal in front always within my sight. During the *séance*, as previously stated, I constantly kept the seal—remaining unaltered—before me on the table. Mr. Slade's hands remained *all the time* in sight; with the left he often touched his forehead, complaining of painful sensations. The portion of the string hanging down rested on my lap,—out of my sight, it is true,—but Mr. Slade's hands *always* remained visible to me. I particularly noticed that Mr. Slade's hands were not withdrawn or changed in position. He himself appeared to be perfectly passive, so that we cannot advance the assertion of his having tied those knots by his *conscious* will, but only that they, under these detailed circumstances, were formed in *his presence* without *visible* contact, and in a room illuminated by bright daylight.

According to the reports so far published the above experiment seems also to have succeeded in Vienna in presence of Mr. Slade, although under less stringent conditions.* Those of my readers who wish for further information on other physical phenomena which have taken place in Mr. Slade's presence, I refer to these two books. I reserve to later publication in my own treatises the description of further experiments obtained by me in twelve *séances* with Mr. Slade, and, as I am expressly authorised to mention, in the presence of my friends and colleagues, Prof. Fechner, Prof. Wilhelm Weber, the celebrated electrician from Göttingen, and Herr Scheibner, Professor of Mathematics in the University of Leipzig, who are *perfectly* convinced of the reality of the observed facts, altogether excluding imposture or prestidigitation.

At the end of my first treatise, already finished in manuscript in the course of August, 1877, I called attention to the circumstance that a certain number of physical phenomena, which, by "synthetical conclusions *à priori*," might

* "Mr. Slade's Aufenshalt in Wien: Ein offener Brief an meine freunde." Wien: I. C. Fischer, and Co., 1878. "Der Individualismus im Lichte der Biologie und Philosophie der Gegenwart von Lazar B. Hellenbach." Wien: Braumüller, 1878.

XUM

be explained through the generalised conception of space and the platonic hypothesis of projection, coincided with so-called spiritualistic phenomena. Cautiously, however, I said:—

> "To those of my readers who are inclined to see in spiritualistic phenomena an *empirical* confirmation of those phenomena above deduced in regard to their *theoretical* possibility, I beg to observe that from the point of view of idealism there must first be given a precise definition and criticism of *objective reality*. Indeed, if *everything* perceivable is a conception produced in us by *unknown* causes, the distinguishing characteristic of the *objective* reality from the *subjective* reality (phantasma) cannot be sought in nature, but only in accidental attributes of that process producing conceptions. If causes unknown to us produce simultaneously in several individuals the same conception, only subject to those distinctions which depend upon differences in the position of the observers, we refer such conception to a *real* object *outside* of us; this conception not taking place, we refer that conception to causes *within* us, and call it hallucination.
>
> "Now, whether the spiritualistic phenomena belong to the first or to the second category of these conceptions, I do not venture to decide, so far never having witnessed such phenomena. On the other hand, I do not possess, with regard to men like Crookes, Wallace, and others, such an exalted opinion of *my own* intellect, as to believe that I myself, under similar conditions, should not be subject to the same impressions." (Written in August, 1877.)

This supposition received, four months after my writing it down, a full confirmation by the above-mentioned experiments with the American, Mr. Henry Slade. In making them I was intent upon giving full consideration to the above-cited distinction between a subjective phantasma and an objective fact. The four knots in the above-mentioned cord, with the seal unbroken, this day still lie before me; I can send this cord to any man for examination; I might send it by turn to all the learned societies of the world, so as to convince them that not a *subjective* phantasma is here in question but an *objective* and lasting effect produced in the material world, which no human intelligence with the conceptions of space so far current is able to explain.

If, nevertheless, the foundation of this fact, deduced by

me on the ground of an enlarged conception of space, should be denied, only one other kind of explanation would remain, arising from a moral mode of consideration that at present, it is true, is quite customary. This explanation would consist in the presumption that I myself and the honourable men and citizens of Leipzig, in whose presence several of these cords were sealed, were either common impostors or were not in possession of our sound senses sufficient to perceive if Mr. Slade himself, before the cords were sealed, had tied them in knots. The discussion, however, of such a hypothesis would no longer belong to the dominion of science, but would fall under the category of social decency.

Some other still more surprising experiments—prepared by me with a view to further testing this theory of space—have succeeded, though Mr. Slade thought their success impossible. The sympathising and intelligent reader will be able to understand my delight caused thereby. Mr. Slade produced on me and on my friends the impression of his being a gentleman: the sentence for imposture pronounced against him in London necessarily excited our *moral* sympathy, for the *physical* facts observed by us in so astonishing a variety, in his presence, negatived on every reasonable ground the supposition that he in one solitary case had taken refuge in wilful imposture. Mr. Slade, in our eyes, therefore, was innocently condemned—a victim of his accuser's and his judge's limited knowledge.

VII. LIQUEFACTION OF OXYGEN.*

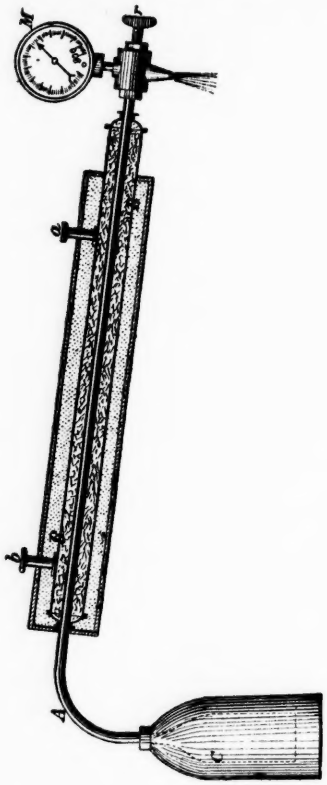
By M. Raoul Pictet.

THE object which I have had in view for more than three years is to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception.

If the permanent gases are not capable of liquefying, we must conclude that their constituent particles do not attract each other, and thus do not conform to this law.

Thus, to cause experimentally the molecules of a gas to

* The *liquefaction of oxygen* is so important a scientific achievement that we have much pleasure in laying before our readers the following detailed account of the means employed and diagrams of the apparatus used, which were communicated to us by M. Pictet himself.—Ed. *Q. J. S.*



DESCRIPTION OF THE DRAWINGS.

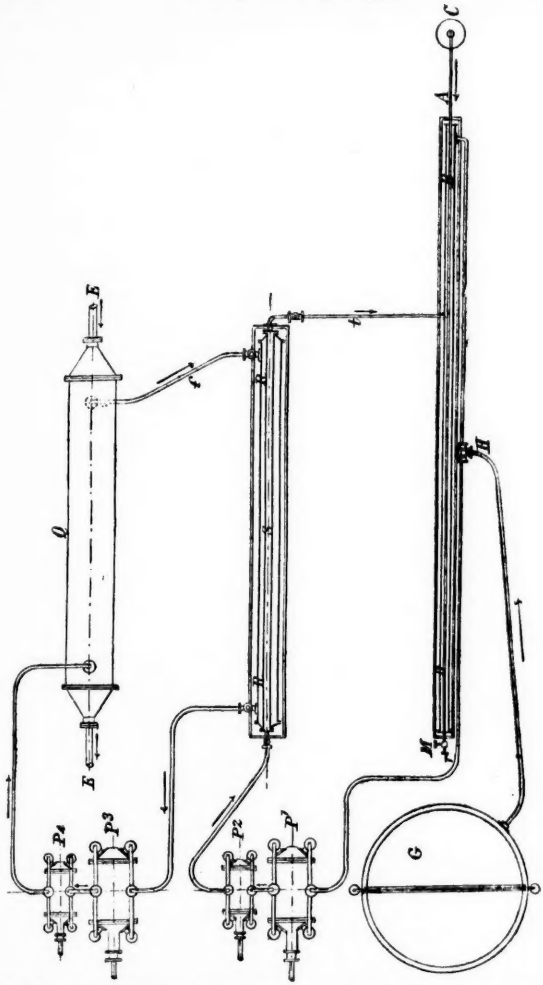
A. A tube, 5 metres long, 14 millimetres external diameter, and 4 millimetres internal diameter, in which the oxygen condenses. It is furnished with a screw-tap, *r*, from which the liquid oxygen jets out. A pressure-gauge, M, measures the pressure up to 800 atmospheres.

B. A tube, 4 metres long, in which is solid carbonic acid. The stock of carbonic acid is contained in a gasometer, G, of 1 cubic metre capacity. A three-way tap, H, puts it when desired into communication with the apparatus.

C. A howitzer shell, containing 700 grms. of chlorate of potash mixed with chloride of potassium. It is heated with gas.

P_1, P_2. Double-action exhaustion and force pumps, drawing carbonic acid from the tube B or the gasometer G, according to the position of the tap H.

S. A tube, 60 millimetres diameter and 1·1 metres long, in which is condensed the liquid carbonic acid compressed by the pumps. This liquefied gas returns by the small tube *t* to the tube B.



R. A tube, 125 millimetres in diameter and 1·1 metres long, containing liquid sulphurous acid.

P3, P4. Double-action exhaustion and force pumps, exhausting sulphurous acid gas from the tube R.

Q. A tubular condenser of sulphurous acid compressed by the pumps. This body, when liquefied, returns by the small tube *f* to the tube R. The cold water for condensing the sulphurous acid passes through the apertures E E.

a. Entry for liquid carbonic acid.

. Exit for the vapourised carbonic acid caused by the suction of the pumps.

approach each other as much as possible, certain indispensable conditions are necessary, which may be expressed thus :—

1. To have the gas absolutely pure, with no trace of foreign gas.
2. To be able to obtain extremely energetic pressures.
3. To obtain intense cold, and to subtract heat at these low temperatures.
4. To utilise a large surface for condensation at these low temperatures.
5. To be able to utilise the rapid expansion of the gas from extreme condensation to the atmospheric pressure—an expansion which, added to the preceding means, will *compel* liquefaction.

Having fulfilled these five conditions, we may formulate the following alternative :—

When a gas is compressed to 500 or 600 atmospheres, and kept at a temperature of $-100°$ or $-140°$, and it is allowed to expand to the atmospheric pressure, one of two things takes place :—

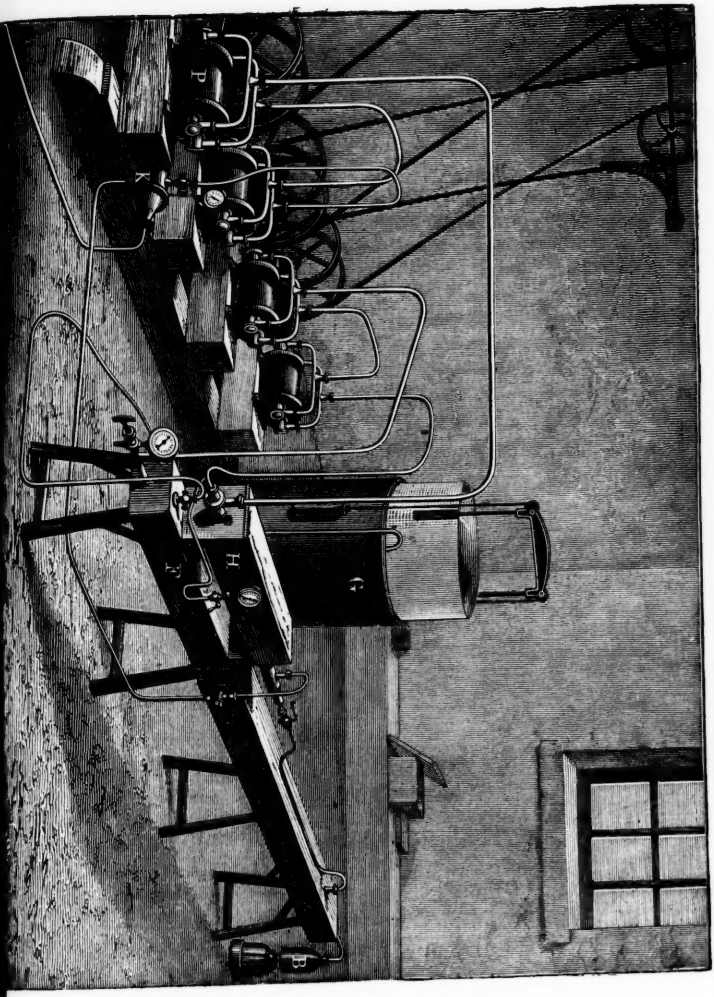
Either the gas, obeying the force of cohesion, liquefies, and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder.

The work done by expansion will not be possible, and the loss of heat will be absolute.

Struck with the truth of this alternative, which is rendered certain by thermo-dynamic equations based on accurate data, I have sought to produce a mechanical arrangement which should entirely satisfy these different conditions, and I have chosen the complicated apparatus of which the following is a brief description :—

I take two pumps, P_3 and P_4, for exhaustion and compression, such as are used industrially in my ice-making apparatus. I couple these pumps in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first communicates with a tube (R) of 1·1 metres long and 12·5 centimetres in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of the liquid rapidly sinks to $-65°$, and even to $-73°$, the extreme limit attained.

Through this tube of sulphurous acid passes a second smaller tube (S), of 6 centimetres diameter, and the same


P
K
F
H
B

length as the envelope. These two tubes are closed by a common base.

In the central tube is retained compressed carbonic acid produced by the reaction of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer (G) of 1 cubic metre capacity.

At a pressure of from 4 to 6 atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube (B), 4 metres in length and 4 centimetres in diameter.

Two pumps, P_1 and P_2, coupled together like the first, exhaust carbonic acid either from the gasometer (G) or from the long tube (B) full of liquid carbonic acid.

The ingress to these pumps is governed by a three-way tap, H. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube; it is situated between the condenser of carbonic acid and this long tube. When this screw valve is closed, and the two pumps draw the vapour from the liquid carbonic acid contained in the tube 4 metres long, the greatest possible lowering of temperature is produced; the carbonic acid solidifies and descends to about $-140°$. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out 3 litres per stroke, and the speed is 100 revolutions a minute.

Both the sulphurous acid tube and the carbonic acid tube are covered with a casing of wood and non-conducting stuff to intercept radiation.

In the interior of the carbonic acid tube, B, passes a fourth tube, A, intended for the compression of oxygen; it is 5 metres long and 14 millimetres in external diameter. Its internal diameter is 4 millimetres. This long tube is consequently immersed in solid carbonic acid, and its whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about 1 metre beyond the other. I have curved this portion downwards, and given the two long tubes a slightly inclined position, but still very near the horizontal, as I have shown in the accompanying drawing.

The small central tube is curved at A, and screws into the neck of a large howitzer shell, C, the sides of which are 35 millimetres thick; the height is 28 centimetres, and the diameter 17 centimetres.

This shell contains 700 grms. of chlorate of potash and 256 grms. of chloride of potassium mixed together, fused,

then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the temperature to the required degree, I heat the shell over a series of gas-burners. The decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. A pressure-gauge, M, at the extremity of the long tube, lets me constantly observe the pressure and the progress of the reaction. This gauge is graduated to 800 atmospheres, and was made for me expressly by Bourdon, of Paris.

When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little, and stops at 320 atmospheres. If at this moment I open the screw-tap, *r*, which terminates the tube, a jet of liquid is distinctly seen to spirt out with extreme violence. I close the tap, and in the course of a few moments a second jet—less abundant, however, can be obtained.

Pieces of charcoal, slightly incandescent, put in this jet inflame spontaneously with inconceivable violence. I have not yet succeeded in collecting the liquid, on account of the considerable projectile force with which it escapes, but I am trying to arrange a pipette, previously cooled, which possibly may be able to retain a little of this liquid.

Yesterday I repeated this experiment before the majority of the members of our Physical Society, and we had three successive jets, well characterised. I cannot yet determine the minimum pressure necessary, for it is evident that I have a surplus pressure produced by the excess of gas accumulated in the shell, and which could not condense in the small space represented by the interior tube.

I hope to utilise a similar arrangement in atempting the condensation of hydrogen and nitrogen, and I am especially occupied with the possibility of maintaining low temperatures very easily, thanks to four large industrial pumps which I have at my disposal, worked by a steam-engine.

The following experiment was performed for the fourth time on Thursday, December 27th, in the presence of ten scientific men—among others, Prof. Hagenbach, of Bâle, who came expressly to assist at this important experiment, the success of which called forth the applause of all present:—

At 10 o'clock in the evening the manometer, which had risen to 560 atmospheres, sank in a few minutes to 505, and remained stationary at this figure for more than half-an-hour, showing by this diminution in the pressure that part

of the gas had assumed the liquid form under the influence of the 140 degrees of cold to which it was exposed. The tap closing the orifice of the tube was then opened, and a jet of oxygen spirted out with extraordinary violence.

A ray of electric light being thrown on the escaping jet showed that it was chiefly composed of two parts;—one central, and some centimetres long, the whiteness of which showed that the element was liquid, or even solid; the other exterior, the blue tint of which indicated the presence of oxygen compressed and frozen in the gaseous state.

VIII. THE PHONOGRAPH.

NO sooner had the wonderful simplicity and marvellous capabilities of Mr. Graham Bell's articulating telephone been practically demonstrated in England than we were startled by an announcement in the American journals that another instrument had been invented, by Mr. T. A. Edison, which would not only receive and register, but also reproduce at any distant period, whatever sounds were uttered into it by the human voice. The first accounts of the wonders of the instrument were evidently somewhat coloured: that it does, however, actually re-produce vocal sounds was demonstrated by Mr. W. H. Preece, at his Lecture on the Telephone, at one of the Friday evening meetings at the Royal Institution, when he exhibited the Phonograph for the first time in England. By the courtesy of the Editor of "Engineering" we are enabled to place before our readers drawings and descriptions of different forms of this instrument.

Fig. 1 is a general view of Mr. Edison's instrument, which has recently been brought to this country by Mr. Puscus, his representative. It consists of a brass cylinder, which, by a winch handle, can be rotated on a horizontal axis, upon which is fixed a heavy fly wheel for the purpose of controlling, to some extent, its speed of rotation. One end of this horizontal axis is screwed, and turns in a screwed bearing, so that the cylinder is not only rotated on its axis, but has imparted to it a lateral movement from end to end when the winch is rotated. Around the circumference of the cylinder

is turned a spiral groove, the pitch of which is the same as that of the screw on the horizontal shaft, so that if a fixed pointer were to be set in the groove at any portion of its

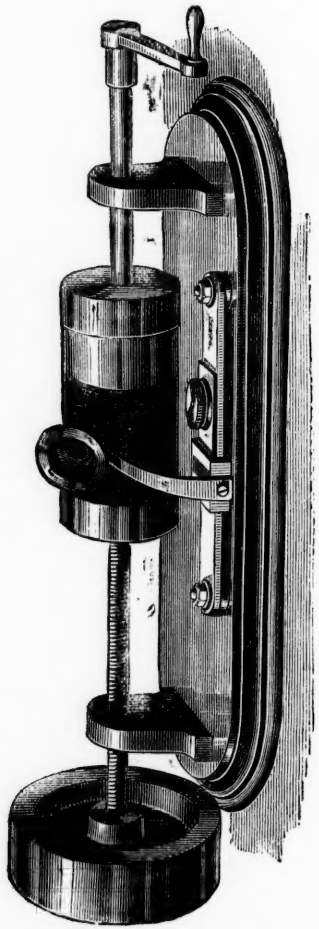


FIG. 1.

length it would remain in it as the cylinder was rotated until it worked out at either end.

In front of the cylinder and directed to its axis is fixed a thin metallic diaphragm, carried by an arm attached to the

stand of the instrument, and provided with adjustments by which a steel pin projecting from its centre may be accurately set in the middle of the groove and at a proper depth; and in front of the diaphragm a mouthpiece is fixed, very similar in form to that employed in Professor Bell's telephone.

From the above description it will be evident that if the diaphragm be set into vibration by sounds being uttered into the mouthpiece, the steel pin attached to it, partaking of that vibratory motion, will enter into greater or less depths into the groove on the cylinder, according as the amplitude of vibration of the diaphragm by which its motion is controlled be large or small. If, while this vibration is going on, the cylinder be rotated in its screwed bearing, the point of the pin will trace out a spiral undulating path of motion within the groove, the amplitude of whose waves will be equal to that of the vibrations of the diaphragm, their length and form being dependent upon the rapidity and character of the undulations of the metallic membrane combined with the surface speed of the cylinder.

In order to obtain a permanent record of this wave-like path a sheet of ordinary tin-foil is fastened round the cylinder, being secured in its place by brass caps, shown on the drawing; and, as the centre of the diaphragm is adjusted so as always to be opposite to the middle of the groove, which is bridged over by the tin-foil, it follows that the pin in vibrating with the diaphragm must indent the tin-foil, which, at any spot below it, is unsupported by the resisting surface of the cylinder, having nothing but a groove behind it, and as the cylinder is rotated a chain of indentations is produced, which is in every particular a record of the sounds which originated them.

So far the apparatus is complete as an instrument for recording sounds, and as such is not superior to many of its predecessors,—such as the very beautiful logograph of Mr. W. H. Barlow, F.R.S., the phonautograph of M. Leon-Scott, or the instruments of Prof. Marey and the late Sir Charles Wheatstone,—but the most wonderful feature of Mr. Edison's phonograph is that it not only interprets its own record, but does so by re-converting it into sonorous vibrations, repeating the sounds, whether articulate or otherwise, in the actual voice in which they were originally communicated to the mouthpiece.

In Mr. Edison's first apparatus this was accomplished by employing a second diaphragm of paper, fixed on the opposite side of the cylinder to the first diaphragm, and thrown

into vibration by being attached by a silken thread to a light steel spring, which carried at its extremity a blunt metallic point, which was held by the elasticity of the spring against the tin-foil covering the cylinder, but with sufficient lightness to allow it to be thrown into vibration as the indentations on the tin-foil passed beneath it.

Mr. Edison has, however, in his more recent instrument, of which Fig. 1 is an illustration, dispensed with this second membrane, making the one metallic diaphragm do double duty, first by receiving vibrations from the voice and impressing them upon the tin-foil, and afterwards by being thrown into vibration by the passing below its projecting pin of the indentations so produced, and thus giving out a repetition of the original sound.

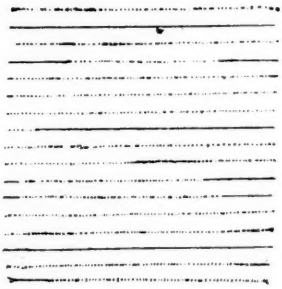
The diagram Fig. 2 will serve to illustrate the principle upon which the tin-foil is impressed by the action of the vibration of the diaphragm, and how the latter is again thrown into precisely similar vibration by the movement of

FIG. 2.

the embossed foil below it. It represents a magnified section of the tin-foil taken along the line of the indentations, showing the position of the pin as it rides over its surface while the foil is travelling below it from right to left. The indentations, of which three only are shown on the diagram, having been produced on the foil by the sonorous vibrations ot the diaphragm, it follows, from the construction of the instrument, that if the foil be drawn under the pin at precisely the same speed as it was travelling when the impressions were made upon it in the first instance, it will cause the diaphragm to vibrate in an exactly similar manner to that in which it vibrated under the influence of the voice, and, from what was pointed out at the beginning of this article, it would therefore emit a similar sound. This diagram is, of course, greatly exaggerated, and must be taken only as an illustration of a possible explanation of what goes on in the action of the instrument, but which is by no means certain. It presupposes that each indentation is made up of a minute structural surface, the details of which are so small as to be quite indistinguishable under

comparatively high powers of the microscope, and yet must be sufficiently pronounced to impart to the diaphragm through its projecting pin those minute variations of vibration by which the proper form is given to the sound-bearing waves of the air, and the exact quality of the sound is conveyed to the ear. It is almost impossible to conceive that microscopical striæ (for they can be nothing more) upon such a substance as tin-foil can impart, by mechanical means, to a diaphragm as rigid as that employed in the phonograph, such niceties of motion; but the phenomena connected with the telephone have shown that metallic diaphragms are capable of imparting to the air, and the human ear is capable of detecting, sonorous vibrations whose amplitude is so minute as to have been altogether unsus-

Fig. 3.



pected before. Some idea of the minuteness of the indentations may be formed from Fig. 3, which is printed from an electrotype cast of a piece of the foil, and is therefore a facsimile on a plane surface of the marks recorded by the instrument.

It will readily be understood that, in order to obtain a perfect reproduction of the original sounds, the tin-foil must travel below the diaphragm at precisely the same speed as it was turning when it was receiving the impressions, and therefore in Mr. Edison's second instrument, which we have been describing, the heavy flywheel was added to render the speed of rotation as uniform as possible; but in turning the instrument by hand it is impossible, notwithstanding this addition, to insure the surface speed of the cylinder being always the same. In order to meet this requirement Mr. Edison has since applied clockwork mechanism for driving

the apparatus, with such marked success that twelve clerks were lately able to take down correctly portions of newspaper articles from dictation spoken to them by the instru-

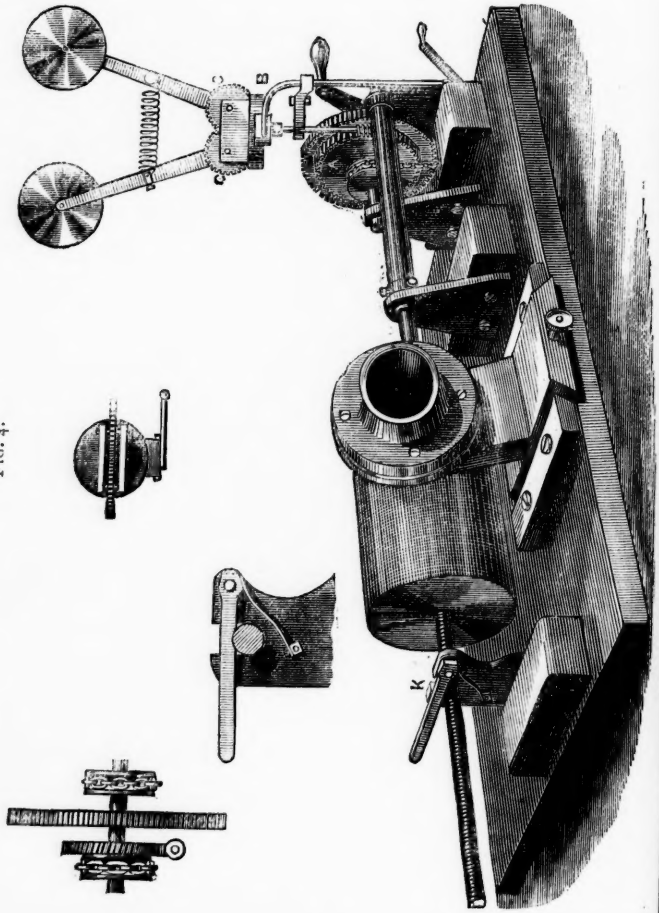


Fig. 4.

ment. At a recent exhibition of the phonograph in New York its articulation was distinctly heard and understood at a distance of 425 feet from the instrument. Still more

recently Mr. Edison has made records on copper-foil that could be read at a distance of 275 feet in the open air.

Fig. 4 is a view of a very beautifully arranged instrument, designed and constructed for his own use and instruction, by Mr. Augustus Stroh.

The cylinder is driven round at a surface speed of about 1 foot in a second, by means of exceedingly simple controlled clockwork mechanism actuated by a descending weight, attached, upon Huyghen's maintaining principle, to an endless chain passing over a pulley fixed upon the principal axis of the instrument, so that it is possible to wind up the weight while the cylinder is rotating without affecting its speed.

The controlling fan, or governor, which is beautifully simple and efficient, consists of two circular disks of brass mounted at the upper ends of two light levers, which are geared together at their lower ends, so as to cause them to fly out symmetrically on each side of the axis of rotation of the vertical fly-shaft to which they are pivoted. When the machine is started (by taking off the pressure of a small cork-lined brake-block, shown in small detail sketch, which presses against the cylindrical head of the fly-spindle) the disks fly out under the influence of centrifugal force, and the resistance of the air to the motion of the spindle is increased by the increase in the diameter of their path of rotation if the speed become too great. Should, however, the speed of rotation tend to fall off, a spiral spring, which can be attached to the fan levers at any position in their length, draws them together, and, by reducing the circle of their path, offers to the mechanism a diminished resistance to rotation.

At a recent meeting of the Society of Telegraph Engineers both the instruments which we here describe were exhibited in illustration of a very interesting paper, by Mr. W. H. Preece, C.E., upon this last and perhaps greatest marvel of the application of Science which this or any other age has seen.

Mr. Edison's first form of phonograph was represented by a very successful instrument made by an amateur, Mr. Pigeon, from descriptions received from America, and in which the two diaphragms, the one of paper and the other of metal, were employed.

The second form of apparatus was represented by Mr. Edison's own instrument (Fig. 1), and the more perfect form driven by controlled mechanism was represented by Mr.

Stroh's instrument, which has been described, and which is illustrated in Fig. 4.

The first words were spoken into the mouthpiece by Mr. Puscus. The mouthpiece was then withdrawn, the cylinder turned back until the pin was at the beginning of the groove, a cone of paper or speaking trumpet was put on in front of the mouthpiece, and the handle once more rotated, when the instrument shouted out, in a perfectly clear voice, "The Phonograph presents its compliments to the audience." This was heard in every portion of the hall of the Institution of Civil Engineers, and brought forth rounds of applause, to which were added roars of laughter, when it again called out in a voice still clearer than before, "How do you do? How do you like the Phonograph?" and then began to laugh in veritable hearty human laughter, "Ha! ha! ha! ha! ha! hurray!"

Mr. Pigeon's instrument was next tried, and the sublime words of the national war song—

"We don't want to fight, but by Jingo if we do,"

followed by the recital of the equally ennobling creation of the poet—

"Twinkle, twinkle, little star,
How I wonder what you are."

were given by the instrument with an emphasis entirely its own, which caused great merriment.

A song was next sung into the mouthpiece, and was reproduced amazingly out of tune, in consequence of the impossibility of obtaining a perfectly uniform speed of rotation.

When, however, Mr. Stroh's instrument was brought into use, the value to the phonograph of controlling the speed of the cylinder by mechanical means was at once apparent, for not only was the articulation of spoken words more perfect, but songs sung into it by Mr. Spagnoletti, Mr. Edmunds, and Mr. Preece, were reproduced with very respectable correctness; and even the breakdown of one of the singers at a high note, accompanied by a little impatient remark, was faithfully recorded, and given out again with exasperating fidelity.

At the Physical Society, on March 2nd, the instrument was again described by Mr. Preece, followed by a similar series of experiments, with the addition of causing the instrument to perform the wonderful feat of reproducing a duet

sung into it through a double mouthpiece by Mr. Spagnoletti and Mr. Sedley Taylor. The result would certainly not make the musical reputation of either gentleman, did it stand upon no intrinsic merits of its own, but it was a remarkable experiment as showing the marvellous powers of which the phonograph of the future may be capable. At this meeting it was further shown that when an indented sheet of tin-foil has been employed to emit sounds it retains its form with such perfectness that the sounds can be reproduced by means of it a second, and even a third, time, with nearly equal distinctness.

NOTICES OF BOOKS.

Some Chemical Difficulties of Evolution. By J. J. MACLAREN, M.A. London: Bumpus.

PERHAPS our readers may remember that about two years ago there appeared a book entitled "A Critical Examination of some of the Principal Arguments for and against Darwinism." Whilst cheerfully acknowledging the candour shown by the author, we had but too much reason to declare that his work was more calculated to retard than to promote the definite solution of the question which he had attempted to discuss. We urged that for men to deal with the most complicated problems in any science before having made themselves familiar with its rudiments was an absurdity at once painful and ludicrous. We have met with no instance where a working naturalist—a man who had really studied organic life in the fields, the woods, the museum, and the biological laboratory—formed a more favourable opinion of Mr. Maclaren's book than did we. But, unfortunately, special knowledge of the subject in question is not considered more necessary for reviewers than for authors. Certain writers in the political and social papers of the day—on the principle, doubtless, that "a fellow-feeling makes us wondrous kind"—pronounced the book "useful and acceptable," and thus encouraged the author to further efforts in the same direction. In our notice of Mr. Maclaren's former work we pointed out that he failed to "distinguish with sufficient clearness between *Evolutionism*—the doctrine of a progressive mutation of species—and *Darwinism*—the explanation of such changes by the hypothesis of Natural and Sexual Selection." In the present volume the confusion is worse confounded, in a manner which we think indicates, to say the least, very culpable carelessness. He tells us that there have been put forward two views—the "Creationists' view," the view of the "Evolutionist properly so-called, besides Mr. Darwin's view." We are not sure whether the author is not here taking leave of that fairness which characterised his first work. To give to the opponents of Evolution the name of "Creationists" seems very like the *suggestio falsi* that Evolutionists as such deny creation. The "Creationists," according to Mr. Maclaren, hold—

(1.) "That life was originally called into existence by a mighty power whose commands matter and force are obliged to obey.

(2.) "That the same mighty power, after having originally called the first living beings into existence, has continued to take a direct part in calling into existence the various new forms which have from time to time appeared on the earth."

It would possibly surprise Mr. Maclaren were he told that there are multitudes of Evolutionists who fully accept the first of these propositions, and, perhaps with the sole exception of the word "direct," which might require some qualification, even the second! On the other hand, there are, or at least have been, men who, whilst disbelieving in the transmutation of species, repudiate equally the necessity of a Creator, asserting that each form of life sprang into existence—

—— "When fatal chance
Had circled its full orb."

The old school of Natural History has therefore no exclusive right to the title of "Creationist," its sole fundamental principles being that species are non-transmutable, and that each form of life originated from inorganic matter.

In opposition to the "Creationist" we have next the "Evolutionist," of whose opinions the author gives a singularly travestied summary:—

(1.) "That the first combination of atoms which possessed the distinctive properties of life was called into existence by the action of the physical forces which still surround us, on a portion of the matter of which our earth consists.

(2.) "That the first form of living being when once called into existence, and afterwards its more or less modified descendants, have been made to vary gradually by changes in the external forces incident; that the actual variations produced were due in part to gradual changes in the external incident forces, and in part to internal variations, the more or less remote consequences of previous changes in the external forces surrounding living beings; that any variation arising in this way which gave the living being in which it occurred an advantage in the struggle for life under the conditions which then prevailed was picked out by the survival of the fittest to compete in this struggle, or, as it is generally called, by Natural Selection; that all the known forms of life, both existing and extinct, have in this manner been developed from the first simple form of living being under the influence of repeated slow changes; and that all change of form among living beings has been very gradual."

Further, we find Darwinianism not placed as it ought as one of the forms of Evolutionism, but classified as a distinct and independent doctrine. If Mr. Maclaren had taken the trouble to master even the outlines of the subject upon which he is, for the second time, coming forward as a teacher, he would have found that the "view of the Evolutionist properly so called" merely amounts to this—*viz.*, that organic species are capable of trans-

mutation, and that new species, like new individuals, take their rise not from inorganic lifeless matter, but from antecedent living beings. Within this common doctrine shades of opinion exist in vast variety, that of Mr. Darwin being one and that which Mr. Maclaren coolly ascribes to "the Evolutionist" another. An Evolutionist may, certainly, in common alike with Mr. Wallace and Mr. Darwin on the one hand, and with Professors Haeckel and Oscar Schmidt on the other, consider that the origin of species is due wholly or in great part to the agency of Natural Selection; or he may, with Mr. Mivart, ascribe it rather to an internal force which occasions modification of structure in certain divinely pre-ordained directions. He may regard Evolution as a slow, gradual process, moving on at one uniform rate; or he may, with Dr. Leconte, ascribe to it a more paroxysmal character, having its periods of permanence followed by shorter intervals of rapid change. Our author, overlooking all these important distinctions, and imputing to all Evolutionists, as a body, notions which many of them would most earnestly repudiate, has, at best, set up a straw puppet to show his skill in demolishing it, and has given most abundant proof of his own inability to handle the subject. The chemical difficulties which he points out, and which might have been summed up in much fewer words, are not devoid of a certain weight as against Haeckel. As against Darwin and Wallace, the author himself is not very confident of their validity, whilst there are other schools of Evolutionists whom they do not touch at all.

We notice the following passage:—"Consider such a compound as the frightful poison aconitine, which is met with only in the roots and other parts of the genus Aconite. This is a distinct and specific chemical compound, and is one of the most powerful and active poisons known, and it will at once be said the possession of such a poison must be of great advantage to the plant as a protection from enemies which would otherwise devour it. No doubt; but the point is whether anything was gained by the elaboration of so very intense and active a poison. Would not the same end have been practically attained by the development of an acrid poison of far less intensity? And, if so, how came it that this special compound was wrought up to so unnecessary a point of perfection, when the energy of the plant would have been more usefully directed into other operations?" But how does Mr. Maclaren know that the production of an intense poison is a greater strain upon the energies of a plant than the secretion of a colouring principle, of an odour, or of a non-poisonous organic base? Further, much of his question can with great effect be turned upon the teleologists of the Old School. Why, for instance, is the bite of the cobra so much more venomous than is required for the destruction of the animals upon which it preys? Against larger animals it is no real defence, for though mortally wounded they have ample time to take summary vengeance.

That chemistry has evidence to give upon the question of

Organic Evolution we do not dispute, and Mr. Maclaren may have rendered some service by calling the attention of enquirers in that direction. But to find out such evidence and to ascertain its value will require the labour of many years—perhaps of more than one life-time.

Annual Report of the Board of Regents of the Smithsonian Institution. Washington: Government Printing Office. 1877.

In the official reports with which this volume commences we find an announcement that the National Museum, which has hitherto been merged in the Smithsonian Institution, is about to be placed on a distinct basis, it being considered that the two establishments have now reached such a state of development that they can scarcely be continued under one common organization. The appropriation for the maintenance of the Museum now amounts to 20,000 dollars annually.

Among the memoirs included in the volume are the *eloge* on Gay-Lussac, delivered by Arago; a biographical sketch of the present Emperor of Brazil; a paper by Mr. W. B. Taylor, of Washington, on "Kinetic Theories of Gravitation." After reviewing all the speculations which more or less directly bear upon the subject from the days of Dr. R. Hooke down to the present time, the author, in summing up, remarks that *every* kinetic system suffers from the "culminating vice" of an "utterly reckless violation of any rational conception of the conservation of energy." He continues: "And yet, remarkably enough, the ostensible impulse and occasion of such creeds have usually been a strong veneration for this much-abused principle and the consciousness of a special mission to restore and to vindicate its neglected authority! Not unfrequently the vibrations communicated to the telegraphic æther by a trembling atom have been supposed to be transmitted unimpaired to that or to other atoms and back again in endless and magnificent cycles of perpetual motion. And as there is no limit to the *vis viva* which such a medium may conserve within its boundless bosom, such projectors have the Bank of the Infinite on which to draw in every dynamic emergency, without the fear of a depleted treasury, and without any necessity being felt for inquiring too nicely into the balance of the depositor's account. And thus, as Leray has intimated, suns and stars are maintained blazing for ever on a borrowed capital of motion."

In opposition to such views the author maintains that we have, from experience, no reason for believing the æther to be in any case a source of energy.

There is also a long and not unimportant paper by Prof. G. Pilar, on the "Revolutions of the Crust of the Earth." The

author starts with the hypothesis of M. Faye, who, in opposition to Wilson, Herschel, and Arago, maintains that the mass of the sun is entirely gaseous, and that by the contraction of this mass is furnished the enormous amount of heat radiated into space. Reviewing the gradual cooling and condensation alike of stars and of planets, he brings us to the dawn of the geological life of our earth, which he describes in accordance with views generally accepted. The elevatory action of the central heat he considers as "slow and constant." In his views of areas of elevation he mentions Australia, as not merely now rising, but as having been covered by the sea at no very distant time. Yet it seems perfectly clear that at dates geologically not very remote Australia must have passed through a stage of subsidence, and that it extended eastwards as far as the great "Barrier Reef," and to have been almost, if not quite, connected with New Guinea to the northward, as has been shown by Mr. A. R. Wallace.

In the next chapter, treating of the "Fluid Envelope of the Earth," he calculates that at the present rate of denudation the entire American continent must be levelled and buried in the sea in four million years, supposing no upheavals to intervene. The denudation of India proceeds at a rate at least three times more rapid.

The author does not consider the atmosphere as permanent either in its mass or its composition. "According to Ebelmann (?), if the stratified rocks had contained 1 per cent. of protoxide of iron, this would have been sufficient to absorb all the oxygen of the air." He maintains that the (free) nitrogen of the atmosphere is also diminishing in quantity. He thinks that nitrogen is "taken out of the air by guano and other analogous deposits;" and, on the authority of D'Archiac, asserts that the nitrogen withdrawn from the atmosphere cannot unassisted return to its source, and that we know of no natural agent for its restoration! By "Swiss Saxony" (p. 310) Prof. Pilar probably means the district usually known as the Saxon Switzerland, or better as the Highlands of the Elbe, extending from the southern side of the Dresden district to the frontiers of Bohemia. Speaking of the fact, recorded already by Pliny, that mineral spring water is unsuited for culinary purposes, the author tells us that "Chemistry teaches us that selenitic waters hold in solution carbonate of lime, which combining with the legumin render these fruit unfit for alimentation." We should rather suppose that selenitic waters must contain selenite, *i.e.*, sulphate of lime.

The author's remarks upon the fossil fauna and flora of the world and upon the origin of species cannot be considered remarkably happy. Speaking of coal, he says:—"England, Belgium, France, Prussia, Silesia, Bohemia, Hungary, possess mines of more or less importance. Scandinavia, Russia, Greece,

Italy have, we may say, no coal-deposits." Here is a serious error; the coal-beds of Russia are large and important. He proceeds:—"Of other parts of the world America is the most richly gifted; while Australia possesses the least." Here we must again convict him of error; there are undoubtedly regions totally without coal, whilst Australia contains very considerable deposits.

The view that the desert of Sahara was once a sea, communicating through the gulf of Gabes with the ocean, is by no means universally accepted.

The author's fifth chapter, on "Ice," deals with the interesting and complicated question of glaciation. We have first the old dispute as to whether the climate of Europe is improving or deteriorating. M. Pilar evidently takes the latter view. He writes:—"We may, on the contrary, attribute to a slow cooling the increased extent of the Swiss glaciers which from the twelfth century have more and more obstructed the ancient passes of the mountains, destroying forests and habitations, and reducing the temperature of the surrounding country. In France and Belgium the culture of the vine has ceased in many regions where formerly its products were of great importance. The disappearance of pines in Ireland indicates that Britain also experiences this decrease of temperature. But the most striking proof of a considerable cooling is furnished us by the dwindling and decay of the birch forests of Iceland. This island, eminently volcanic, was in the middle ages the seat of an advanced civilisation. The magnificent woods were peopled with numerous animals, and the nightingale made music in the groves of this island now ravaged by cold and fire." This passage he substantially repeats elsewhere, maintaining that the temperature of the northern hemisphere has been decreasing since A.D. 1248, whilst that of the southern has been improving. Upon these supposed changes is based to a great extent his adhesion to the famous theory of Adhémar. We must therefore examine the alleged facts a little more closely. The Swiss glaciers, so far from having been gradually extending since the twelfth century, are now well known to be shrinking, and have in many striking cases a much less extent than when Switzerland was first recognised as the "play-ground of Europe." As regards Ireland, where fuschias and myrtles—and, according to the author, even palm trees—flourish in the open air, it is very certain that the disappearance of pines, if a historical fact, cannot be due to a fall of temperature. The position and circumstances of Iceland are so peculiar that the decay of the birch forests there is not a sufficient proof of the deterioration of the climate. The demands of the inhabitants for fuel, the action of the tremendous volcanic outbursts which desolated the island, and the ravages of goats, may amply account for the decline of these forests, and when trees are once swept away from any country, by what

cause soever, their re-introduction is a difficult task. The Iceland nightingales are probably due to the imagination of the poets to whom Professor Pilar appeals. We can scarcely suppose a bird of such limited powers of flight able to cross the wide and stormy seas which intervene between Iceland and either Norway or Scotland. In his hypothesis of a deluge every 10,500 years, proceeding alternately from the north and from the south, M. Adhémar not only assumes the existence of polar ice-caps larger than observation justifies, but takes for granted that they rest upon the bottom of the ocean. Nor is the supposition of one unbroken ice-cap covering the whole of Northern Europe scarcely tenable in the face of the observations of Mr. Mattieu Williams.* Whilst fully admitting also that submergence and emergence have alternated over many, if not all, parts of the surface of our globe, can we recognise the probability of a universal deluge, previous to which the preponderance of water was in the northern hemisphere, having occurred less than 10,000 years ago. All that we know of the distribution of organic life, as well marine as terrestrial, seems to point to the conclusion that for a very prolonged period land has predominated in the northern hemisphere and water in the southern.

A Treatise on the Cycloid and all Forms of Cycloidal Curves. By Richard A. Proctor. London: Longmans and Co. 1878.

This is a work or two hundred and fifty pages, devoted to the investigation of a family of curves which have lost most of the interest which formerly attached to them, and which, so far as they are worth knowing, are studied with far greater ease and perspicuity by the analytical method. The mass of details of which the book is composed is hardly relieved by any real generality in treatment in spite of the parallel sequence of the propositions relating to the different curves. An effort is made at the close of the work to exhibit a problem in whose solution a knowledge of the properties of the cycloid is capable of application; but even the approximate solution of Kepler's problem there given fails to account for the fact that "students at the Universities" are expected to find the work of "use."

The plates, which are due to Mr. Perigal and Mr. Boord, are excellent. The beauties and variety of these curves, together with the ease of their mechanical description, will always make them favourites with the amateur turner; but the interest of the mathematician is soon satisfied with noting the effect of changes in the mode of their generation on their form, without enquiring

* Quarterly Journal of Science, vol. vii., p. 537.

into such recondite properties as those referring to the centre of gravity. More plates and less letter-press would have made this book more useful to the designer and certainly not less useful to the student.

Photographed Spectra. Printed by the Autotype Process. By G. Rand Capron, F.R.A.S. London: E. and F. N. Spon. 1877.

This work contains no less than one hundred and thirty-six photographs of metallic and gaseous spectra, accompanied by a full description both of the conditions of each experiment and of the spectra themselves. The spectra range from about the line *b* to beyond H_2 in the violet, and the method of reproduction enables much more of the spectrum in the direction of the violet to be delineated than is ordinarily found in representations of spectra. Each spectrum in its photographed form was found to present a readily recognised individuality, and the author was thus induced to imagine that the permanent reproduction of them might furnish a handy book of reference to spectroscopists.

The spectroscope employed was by Browning, and was specially constructed for auroral observations. The prism was an inch aperture compound, easily dividing the D lines, and the collimator carried a 1¼-inch lens of 6 inches focus. The camera was in immediate connection with the spectroscope, and the images were taken upon collodion wet plates, 4¼ by 3¼ inches. The metallic spectra were produced by the spark, and also by the electric arc. The former was obtained from a large Ruhmkorff coil giving a 2-inch spark, and the latter by forty pint Grove cells. The width of the slit averaged 0·003 inch. Several attempts were made to obtain photographs of the red end of the spectrum. A full explanation of the various photographs is given. All the elements, metallic and non-metallic, with a few exceptions which could not be photographed, are given.

The work will be found of great service to the spectroscopist, and of interest even to those who do not work practically.

Miscellaneous Papers connected with Physical Science. By Humphry Lloyd, D.D., D.C.L., Provost of Trinity College, Dublin. London: Longmans. 1877.

This work contains a reprint of twenty-three papers, reprinted from the "Transactions of the Royal Irish Academy," the "Reports of the British Association for the Advancement of Science," and elsewhere. Four of these relate to Optics, and

include the important paper on Conical Refraction; twelve relate to Magnetism; two to Meteorology; and five are Addresses delivered on various occasions, on various subjects. The papers extend over many years: we have a Report on the Progress and Present State of Physical Optics, which was read before the British Association in 1834; and the Introductory Address delivered before the same Association twenty-three years later. The Report on Physical Optics extends over 128 pages, and is of much value for reference. The magnetic observations go back as far as 1834, and they were made in conjunction with Capt. Sabine and Capt. James Ross. The observations are accompanied by some useful charts. The chapters on the Meteorology of Ireland will always be valuable, and will no doubt form the basis of a complete system of meteorological observation, which may hereafter be established in the island. The Introductory Lecture, on the rise and progress of mechanical philosophy, contains an interesting historical account of the earlier researches in this branch of natural philosophy. Altogether the work will be found of great interest to the man of Science.

Physiography: an Introduction to the Study of Nature. By J. H. Huxley, F.R.S. London: Macmillan. 1877. 8vo., 377 pp.

This work embodies the subject-matter of a course of educational lectures delivered nine years ago at the London Institution. It is a Physical Geography containing the newest and most accurate details, and divested of the formal treatment which usually characterises works on that subject. The opening chapter treats of "The Thames," and this river was chosen because the lectures were addressed to Londoners by a Londoner; but the author points out that any intelligent reader or teacher will have no difficulty in transferring the ideas herein expressed from the Thames to the river and river-basin of his own district. The general formation of rivers is traced, and the nature of watershed and water-parting, land-drainage and springs, is discussed. A map shows the principal river-basins and water-partings of Great Britain.

The second chapter is on "Springs," the formation of which is illustrated by some excellent woodcuts. It is shown that all such sources of water owe their origin to the rain which sinks into the earth, and descends until it finds an impermeable stratum. This naturally leads (Chap. III.) to an account of rain and dew. The formation of clouds is discussed, and illustrated by some good chromo-lithographs; a coloured hyetographical map shows the annual rainfall in different parts of

England, and the formation of dew is described. The account of liquid water is followed by that of solid water (Chap. IV.), and the principal physical properties of ice and snow receive full attention. A chapter on evaporation shows the connection between it and the rainfall.

The sixth chapter is devoted to the atmosphere, the chemical nature of which is fully discussed. This is followed by an account of its physical properties, and of the perturbations which arise owing to alterations in the pressure. "The Times" weather-chart is reproduced and described; also the barometer-charts of the "Standard" and "Daily Telegraph." The chemical and physical history of the air is followed by a chapter in which the composition of pure water is demonstrated. The constituents of mineral waters and of sea-water are discussed, and analyses given.

The ninth chapter is of a more geological character: it treats of the work of rain and rivers, the power of running water, and processes of denudation. The Thames is said, on the authority of Prof. Geikie, to discharge annually 1,865,903 cubic feet of sediment, to which must, of course, be added the mineral matter carried away in solution, which brings up the amount to 14,000,000 cubic feet of solid matter. "Imagine a huge die-shaped mass of stone 100 feet in length, 100 feet in width, and 100 feet in height: this would contain one million cubic feet. No fewer, then, than fourteen of these gigantic cubes appear to be quietly stolen from the surface of the Thames basin by means of running water, and transported to the sea, in the course of a single year. But the Thames basin covers a very large area, and it will be found on calculation that, admitting the abstraction of this vast mass, the entire surface of the basin would be reduced in level by only 1-800th part of an inch every year. At the present rate of wear and tear, therefore, denudation can have lowered the surface of the Thames basin by hardly more than an inch since the Norman Conquest; and nearly a million years must elapse before the whole basin of the Thames will be worn down to the sea-level." Prof. Geikie has calculated that, at the present rate of denudation, it would require 5½ million years to reduce the British Islands to a level with the surface of the sea.

The tenth chapter is devoted to "Ice and its Work:"—the mechanical expanding work of water in the act of freezing, the motions of glaciers, and so on. A capital engraving of the glacier of Zermatt is given on page 156. It is shown that the passage of a glacier across a country produces peculiarities which are not caused by any other process of denudation, and it is thus possible to infer with certainty that ice has been at work in a district in which ice is now never seen. The flat-domed hillocks known as *roches montonnées* are thus produced, and they may be detected in Ireland, Cumberland, Scotland, and North Wales, together with *bloce perchés*, and vestiges of old moraines.

The chapter on the "Sea and its Work" is illustrated by a capital section of the Atlantic between Sandy Hook and Bermuda, in which the soundings are given at short intervals, the temperatures, and the position and dimensions of the Gulf Stream. Here, also (p. 182), we find a chart of the estuary of the Thames between the Nore and Margate. The account of volcanoes and earthquakes includes geysers, and is illustrated by a representation of the Beehim Geyser of Yellowstone Park, Colorado, which throws jets of hot water to a height of 200 feet. It is said that there are no less than 10,000 hot springs, geysers, and hot lakes within the area of Yellowstone Park.

The slow movements of the land (Chapter XIII.) are shown to be altogether more important than the sudden paroxysmal changes produced by earthquakes. The best-known example of such changes within the memory of man is perhaps to be found at Puzzuoli, where the land near the Temple of Serapis appears to be sinking at the rate of one inch in every four years. This occurs in the midst of a volcanic district; but in Scandinavia, a country peculiarly free from earthquakes, we have positive proof of similar slow changes. The northern part of the peninsula is rising, while the southern part appears to be undergoing depression. Evidences of similar changes are not wanting in some parts of Great Britain.

A long chapter (XIV.) is devoted to "Living Matter, and the Effects of its Activity on the Distribution of Terrestrial Solids, Fluids, and Gases. Deposits formed by the Remains of Plants." Herein it is shown that the gaseous and liquid constituents of the earth are being constantly reduced to the solid form, either temporarily or permanently, by living matter. Prof. Huxley objects to the term organic matter, "because all forms of living matter cannot be strictly said to be organised." Vegetable life is traced up from its most primitive beginnings, by a gradual process of evolution, until it attains full perfection, and then passes to decay. The same is done with animal life, commencing with the egg; and the analogy between the growth of the plant and of the animal is shown. Then the principal fossil vegetable forms are described, and the formation of the coal-measures. The whole of this chapter is treated in an original and highly suggestive style. A natural continuation leads, in Chapter XV., to the formation of land by animal agencies, such as "Coral Land," and (Chapter XVI.) "Foraminiferal Land." The latter embraces an account of some of the deep-sea soundings of the *Challenger*, the bed of the Atlantic, and globigerina-ooze.

Since at the commencement of the volume we find an account of the surface configuration of the Valley of the Thames—that river being chosen for the reason stated above—it is appropriate that now the geological structure of the Thames basin should be discussed (Chapter XVII.): this is accompanied by a geolo-

gical map and section of the district. The history of one single river-basin leads to a more general account of the distribution of land and water over the surface of the globe (Chapter XVIII.)

The penultimate chapter treats of the figure of the earth and the construction of maps, and the final chapter of the sun.

It will be seen from the above that no formal sequence of subject-matter has been attempted by the author. A book which commences with "The Thames" and ends with "The Sun," and discusses midway geology, palæontology, and physical geography, interlaced with chemistry and physics, cannot be regarded as an example of great continuity of structure and design. Only a most comprehensive and original mind, such as that of Prof. Huxley, could make such a work at once intensely interesting and highly instructive. The book will be welcomed alike by the general reader, the teacher, and the student. For the upper forms in our modern school divisions it is admirably suited. Its great value to every class of reader depends not only on the matter which it contains, but upon its suggestiveness and upon its excellent literary style.

An Elementary Treatise on Physics, Experimental and Applied. Translated from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D., F.C.S. Eighth Edition, Revised and Enlarged. London: Longmans. 1877.

This is a new edition of a very well-known work which has been reviewed at length in our columns. It is not often that a scientific book of this magnitude reaches an eighth edition in this country, and the fact speaks for itself without any further comment. The present edition contains sixty pages of new matter and sixty-two illustrations. All the most recent discoveries in Science have been introduced, and the work is a complete exponent of the present state of general elementary physics.

A Star Atlas for Students and Observers. Showing 6000 Stars and 1500 Double Stars, Nebulæ, &c., in Twelve Maps on the Equidistant Projection; with Index Maps on the Stereographic Projection. By Richard A. Proctor. Fourth Edition. London: Longmans. 1877.

Three editions of this work having been sold rather rapidly, the author determined to bring out a cheaper edition, and this is the result. The maps are constructed in reference to the year 1880, and they will continue to be more and more correct than in the

year when first published, until 1890; moreover, it will not be as far from correctness, on account of precession, as existing Star Atlases, until the year 1927! The stars which are mapped include all down to the sixth magnitude inclusive, and are taken from the B. A. Catalogue and all other most available sources. The maps are drawn by Mr. Proctor himself. We predict a long-continued career of usefulness to this laborious work.

Transits of Venus. A Popular Account of Past and Coming Transits. Third Edition.

The Universe of Stars, Presenting Researches into, and New Views respecting, the Constitution of the Heavens. Second Edition.

Other Worlds than Ours. The Plurality of Worlds Studied under the Light of Recent Scientific Researches. Fourth Edition.

By RICHARD A. PROCTOR. London: Longmans. 1878.

THESE works are second, third, and fourth editions of well-known treatises on popular astronomy, by that most indefatigable and industrious writer Mr. R. A. Proctor. In each instance the former editions have been revised, and in some respects modified. New discoveries have also been introduced, and these have increased as much in astronomy as in any other science. Of recent matters we have Dr. H. Draper's discovery of oxygen in the sun by means of its bright lines; also the discovery of the two small moons of Mars, which revolve around him in respectively 30¼ and 7⅝ hours, at distances of 14,000 and 5600 miles from his centre. A new star, which appeared in Cygnus, has faded into a planetary nebula, and it emits the monochromatic light of certain gaseous nebulæ whose spectrum is a single nitrogen line. The calculations regarding the distance of the sun from the earth, founded on the recent observations of the Transit of Venus, appear to be drawing to a close, and they give an approximate distance of 93,000,000 miles. Mr. Proctor unites much learning with an easy popular style: he thoroughly understands his subjects, and has carried out much observational research; moreover, he is an excellent draughtsman, and frequently gives us his own designs. Thus his books are at the same time popular and scientifically accurate, and they must always commend themselves to every class of reader.

Pocket Altitude Tables. Short and Simple Rules for Accurately Determining Altitudes Barometrically. By G. J. Symons. London: Stanford.

We once heard of an amateur who fell to work to measure the height of one of the loftiest peaks of the Carpathian range. He made his observations with great precision, calculated out the results, and found—a height, or rather depth, considerably below the sea-level! Mr. Symons's little book will, we think, effectually prevent such an undesirable consummation. By following the author's instructions fairly accurate results may be obtained with a minimum of time and trouble, and without the necessity of employing two observers and two sets of instruments, which on an exploring expedition is not always practicable.

How to Work with the Spectroscope. A Manual of Practical Manipulation with Spectroscopes of all Kinds, &c. By John Browning, F.R.A.S., &c. London: John Browning. 1878.

This little work, from the pen of our leading spectroscope maker, will be found useful to the student who is commencing the study of spectroscopy. It gives practical directions for working with spectroscopes of all descriptions, from the simple pocket form to the large compound prism apparatus. The various kinds of instruments at present in use are also figured and described, so that the beginner may gain an insight into the cost of an outfit before taking up this branch of scientific study. The paragraphs giving directions for mapping spectra might have been extended with advantage. The whole is illustrated with thirty woodcuts and diagrams of apparatus and spectra.

As an introduction to the more expensive works of Schellen, Roscoe, and others, Mr. Browning's little book may be safely recommended, especially if used in conjunction with Mr. Proctor's shilling manual of the Spectroscope.

Mineralogy. By J. H. Collins, F.G.S. Vol. I., The General Principles of Mineralogy. London and Glasgow: W. Collins, Sons, and Co. 1878.

This book, which forms part of Collins's Advanced Science Series, has been specially written to enable those practical working miners, quarrymen, field geologists, and students of the

Science Classes of the Department of Science and Art, who wish to submit to the Government examinations, to pass with credit. The present volume treats more especially of the physical characters of minerals generally, eighteen of the twenty-seven chapters being devoted to a description of Miller's system of crystallography, all mathematical formulæ being avoided. The other physical properties of minerals—such as cleavage, structure, magnetism, optical properties, &c.—are also well described; the analysis of minerals by the blowpipe is, however, dismissed in ten pages. There are but few elementary manuals of chemistry in which any space is given to blowpipe analysis. It seems a pity, therefore, that the opportunity was lost of giving a more detailed account of a subject of such vital importance to the working mineralogist. The use of the spectroscope in the qualitative analysis of minerals is hurried through a dozen lines. We fear that the system of publishing books in series of volumes, each containing an exact number of sheets, is a mischievous one, for in too many instances it compels authors to restrict certain portions of their work within inconvenient limits. The work is illustrated by nearly six hundred diagrams. The second volume, which will shortly be published, will treat of descriptive mineralogy.

Building Construction, showing the Employment of Timber, Lead, and Iron Work, &c. By R. Scott Burn. Vol. I., Text; Vol. II., Plates. London and Glasgow: W. Collins, Sons, and Co. 1878.

The fame which Mr. R. Scott Burn has gained for his numerous works on the constructive arts renders it almost unnecessary to say very much about the latest production of his pen and pencil. We need only open it at any page to see that it is written by a thoroughly practical man, possessed of the power of explaining his meaning in clear homely language. The text is illustrated by nearly five hundred woodcuts, besides which there is a quarto volume of plates, containing thirty pages of plans, working drawings, and diagrams, illustrative of the text. Amongst them are also a number of plates containing examples explanatory of the excellent instructions for mechanical and free-hand drawing.

We cordially recommend these volumes to all who are connected with the constructive arts. The work forms part of Collins's Advanced Science Series.

Botany: Outlines of Morphology and Physiology. By W. R. McNab, M.D., F.L.S., Professor of Botany, Royal College of Science, Ireland. London: Longmans and Co. 1878.

This little work forms part of the series of London Science Class-Books, edited by Prof. G. Carey Foster and Mr. P. Magnus, and is stated to be intended for pupils who have already acquired a rudimentary knowledge of botany. The growth of cells of different kinds, their aggregation to form tissues, and the general external conformation of plants are treated of in the first three chapters. The next five are devoted to the nutrition of plants, their general conditions of life and growth, their movements and modes of reproduction. The book ends with a chapter on classification. So far the general arrangement, which is excellent; but we regret to see a constant tendency on the part of the author to use terms derived from the Greek and Latin, most of them neologisms, when English ones would serve just as well. What possible end can it serve to call the stem of a plant a *caulome*, the leaf a *phyllome*, and its hairs *trichomes*, and that, too, without giving the pupil the least clue to the derivation of these and a hundred other similar words? We must really warn teachers against using a book for any class of school-boys whose chief end seems to be to cram their minds with such facts as these—that "imperfect self-fertilising flowers are kleistogamous;" that "flowers fertilised by the agency of birds are called ornithophilous;" or that "in sympodial dichotomies the sympodium consists either of the fork-branches of the same side, right or left, the bostrychoid (helicoid) dichotomy, as seen in the leaf of *Adiantum pedatum*; or the branches are alternately right and left, the cicinnal (scorpioid) dichotomy of many of the *Selaginellas*." We thought that pedantic cramming was being speedily eradicated from our educational system, but the noxious plant seems to be still flourishing.

A Treatise on Photography. By W. de Wiveslie Abney, F.R.S., &c. Text-Books of Science. London: Longmans and Co. 1878.

Captain Abney has already earned the gratitude of both professional and amateur photographers by his little work "Instruction in Photography," published some twelve months since. His first book, however, was almost entirely practical in its nature, the theory of photography being only treated of incidentally. The present work fully supplements the first, and enters into full details with regard to the theory of the subject. It must not be thought that the author confines himself entirely

to theory, for there are sufficient technical instructions for all the best known wet and dry processes to enable anyone possessing the book to become an accomplished landscape photographer. The history of photography—from the days when Scheele first saw *luna cornua* blackening in the sun, to the latest improvements in photo-engraving processes—is succinctly given in the first chapter; after which the author gives a series of experiments on light, which form an admirable introduction to his third chapter, which treats of the theory of sensitive compounds. This chapter is undoubtedly a tough one, but it will fully repay any amateur or professional for the time and trouble which he may take in mastering it. The next two chapters—on "The Action of Light on Various Compounds," and on "The Support and Substratum"—may be looked upon as a continuation of the third. We are now introduced to the more practical part of the subject, the daguerreotype being the first process treated of. Some practical photographers may smile at this exhumation, but they are perhaps not aware that it was employed by the French astronomical expeditions which were sent out to observe the transit of Venus in 1874. Where minute measurements of the photographic image have to be made, as in astronomical or spectroscopic work, the advantage of having a rigid immovable surface to work on, instead of an unequally contractile film, will be readily understood; in fact, we are surprised that so many photo-spectroscopists should still adhere to the wet or dry collodion process. This being the case, the chapter in question might have been longer. The various collodion processes are next described, the practical instructions being very copious. As might have been expected, they take up a large portion of the book. The gelatino-bromide process and the old-fashioned catotype, which Capt. Abney informs us is still practised in remote districts in India, next claim our attention. We next come to the various printing and toning processes, both on glass and paper; printing with ferric, uranic, and chromic salts, the various autotype processes being as fully described as the secrets of trade will allow. The Woodburytype process is also well described. The next chapters are devoted to the photo-lithographic processes of Col. de C. Scott and Sir Henry James,—much used by the Ordnance Department for the reproduction of enlarged or reduced copies of charts and maps. It may interest our readers to know that large numbers of maps reduced by a modification of this process, introduced by Captain Ali Bey, the chief of the Photographic Department of the Seraskierate, were largely and profitably used by the officers of the Ottoman army engaged in the late disastrous campaign. Photo-engraving and relief processes are next described, but of course practical details are wanting, so many of the operations connected with them being kept secret. A print from a photo-relief plate, by Warnerke, produced by a secret process, is given,

and is one of the best specimens of the kind that we have seen. The two chapters on Lenses and Apparatus will be of great assistance to the amateur purchaser, who frequently wastes an immense deal of time and money through being ignorant of his real requirements. The chapter "On the Picture" is a most valuable one from an artistic point of view. Not only are minute instructions given for obtaining artistic pictures instead of mere photographic transcripts, but the photographer is shown what is right and wrong by means of nearly a dozen beautifully executed woodcuts of scenery of all descriptions, taken from photographs by Manners Gordon, Woodbury, H. P. Robinson, and other masters of the craft. A chapter on actinometers and actinometry follows, and the remainder of the book is devoted to photo-spectroscopy, celestial photography, micro-photography, and the miscellaneous applications of photography.

We most cordially recommend Capt. Abney's book to our readers. We regret to say that the Index to this important work is so meagre as to be almost useless.

A Critical Examination of the Flints from Brixham Cavern. By A. WHITLEY. London: Hardwicke and Bogue.

THIS pamphlet, which is a reprint from the "Transactions of the Victoria Institute," has for its object to disprove that the flints found in the Brixham Cavern were knives, or otherwise show traces of human labour. The author brings to his task no small ingenuity, and a too obvious desire to make the most of every circumstance upon which the faintest doubt may be founded. This is the weak side of his argument; we cannot trust a critic who speaks not as a judge, but as a most passionate advocate. That he has established a charge of carelessness against some of the discoverers and custodians of the relic in question is undeniable.

Zoology of the Vertebrate Animals. By ALEX. MACALISTER, M.D. London: Longmans and Co.

THIS treatise is one of a series entitled the "London Science Class Books," edited by Messrs. G. C. Foster and P. Magnus. According to their Preface, these gentlemen consider that there is "still a want of books adapted for school purposes upon several important branches of Science." Their object being to supply this want, they have sought to obtain the co-operation of men who "combine special knowledge of the subjects on which they write with practical experience in teaching."

Within the very brief space allotted—some 120 small pages—the author has given a fair, but of course very sketchy, account of his subject. We are somewhat surprised to find it stated that the common viper is "easily recognised by its dark green colour." Among the hundreds we have captured, from England to Dalmatia, we never met with a specimen that could be called green; the males were various shades of grey, and the females copper-colour. We fear the author's estimate that over 10,000 deaths take place annually from snake-bites is far too low. As an oversight, we may also notice that the swift is ranked among the birds which leave us "about the first week in October."

Report of the Board of Health of the City of Nashville for the Year ending July 4th, 1877. Nashville: Tavel, Eastman, and Howell.

In addition to a Report on Sanitary Reform in Nashville, this volume comprises papers on the topography, the geology, the water-supply, and the climate of the city. Nashville is a place whose sanitary history, as here given, is exceedingly instructive. It was once a summer health resort. By neglect of proper precaution and regulation during its increase from a village to a city—as has happened in too many places on both sides of the Atlantic—its death-rate increased; but now intelligent attention has been awakened, the tide is turning. Mention is made of one J. M. Bass, who, "as Receiver, replaced the entire City Government," and who "made the fatal mistake of economising at the expense of the public health." It would be well, indeed, if so-called economists, who are always counting the cost of any projected measure, would be fair enough to add the reverse of the medal, and count the cost of letting things alone; it would often prove much the heavier. We find here a high compliment paid to our country of which we are scarcely worthy. The Report speaks of "Great Britain, the acknowledged leader in all sanitary reform." When we read these words we remembered the multitudes of inhabited cellars in our English towns—an abomination entirely absent in Paris.

The following facts, if no mistake prevails, must rank among the unsolved mysteries of medical science:—"that cholera is never seen in Iceland, Siberia, Greenland, or Australia; that phthisis is never seen in Iceland, and only rarely in Norway, Madras, or the elevated plains of Mexico."

We are glad that the sanitary value of trees is fully recognised in this Report, and that planting is urged as a public duty.

A List of Writings relating to the Method of Least Squares, with Historical and Critical Notes. By MANSFIELD MERRIMAN, Ph.D.

A BIBLIOGRAPHICAL treatise of great value to mathematicians, physicists, and engineers.

Sketch of the Origin and Progress of the United States Geological and Geographical Survey of the Territories. By F. V. HAYDEN, U.S. Geologist in Charge. Washington: Darby and Duvall. 1877.

THIS pamphlet contains an interesting and useful sketch of the rise and extension of that continuous scientific exploring condition for which the Federal Government is deservedly reaping golden opinions. The Survey it appears took its origin in 1867, when Nebraska was admitted as a State of the Union, the unexpended balance of the sum appropriated for its legislative expenses as a territory being set apart by Congress for a Geological Survey of the new State. The utility of such an undertaking, executed in a spirit of thoroughness, having become evident to the Legislature (whose wisdom in this respect might be advantageously imitated in other countries), further sums of money were voted, and the Survey was gradually extended, not alone as to the country to be explored, but also as to the subjects included. Not alone the geology—using the term in its widest sense—but the ethnology, the natural history, the meteorology, and the agricultural resources of the territories are carefully studied and reported on. Men of proved eminence are appointed to the various departments, and they are allowed both time and appliances to carry out their task in a manner creditable to themselves and their country, and highly useful to men of Science throughout the civilised world.

Among the most valuable results of the Survey may be mentioned the observations glanced at in the following passage:—"Accumulated experience has shown that the various evolutional tides of organic life have not advanced at the same *rate* in all parts of the world. Thus while we find that a certain grade of vertebrates, invertebrates, and plants are associated together in the strata of, and collectively characterise a certain geological period in, Europe, in America we find that the same grade of plant life was evidently reached much earlier, and the same grade of vertebrate life was continued much later."

One of the features of this Survey has been the employment of a skilful photographer, Mr. W. H. Jackson, whose success in

conveying the characteristics of rocks and mountains we have on a former occasion had the pleasure of pointing out. The value, or rather the necessity, of photography in scientific exploration is scarcely even yet fully understood by the public at large. We are justly told that "twenty years ago hardly more than caricatures existed, as a general rule, of the leading features of overland exploration. Mountains were represented with angles of 60 degrees inclination, covered with great glaciers, and modelled upon the type of any other than the Rocky Mountains; the angular lines of a sandstone mesa represented with all the peculiarities of volcanic upheaval or of massive granite."

The total number of negatives in the possession of the Survey amounts to nearly four thousand.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iii., No. 4; Vol. iv., No. 1. Washington: Government Printing-Office.

THIS number is devoted to zoology, recent and fossil. The first paper, by Mr. S. H. Scudder, contains a description of certain fossil insects discovered in the Tertiary beds of White River, on the borders of Colorado and Utah. With the possible exception of four specimens from the Miocene of North Greenland, they are the first insects found in the Tertiary strata of America. There are in the collection no Lepidoptera, nor has the author yet met with any fossil species of this order of American origin, and no Orthoptera. More than one-half the species are Diptera, thus showing that these creatures—whose absence would be one of the essential requisites of a "golden age"—must have existed from a very early date in the Western Hemisphere. There are three Hymenoptera, four Hemiptera, one Neuropterous insect, and nine Coleoptera. The relative proportion of insects of different orders found in various deposits is of course a document of the highest value as regards their origin. If we consider that the Diptera, from their fragility, are about the least likely insects to be preserved, it will, we think, be allowed that in the Tertiary times they must have been relatively more abundant than in the present day. Among the Coleoptera the absence of Buprestidæ is remarkable.

There is a description of two fossil Carabs found in the Interglacial deposits of Scarboro' Heights, Toronto; a catalogue of the insects collected by Dr. Uhler during the explorations of 1875, with notices of their localities, times of appearance, &c. There are also papers on *Cambarus Couesi*, a new crawfish from Dakota; on a carnivorous Dinosaurian from the Dakota beds of Colorado; a notice on the ichthyological fauna of the Green River Shales; and an account of the genus *Erisichthe*.

The first number in Vol. iv. gives the records of much good work. The first paper, "Notes on the Ornithology of the Lower Rio Grande of Texas," by G. B. Sennett, may give the general public an idea of the difficulties which naturalists have to encounter on their explorations. "While we were constantly on the alert for huge rattlesnakes, tarantulas, and centipedes, still more troublesome enemies were with us continually in the shape of wood-ticks and red bugs, to say nothing of the fleas. The wood-ticks we could pick off or dig out, but the abominable red bugs, too small to be seen, work themselves through the clothes and into the skin, making one almost wild with incessant itching. We only obtained partial relief by giving ourselves from head to foot, before going to bed, a bath of ammonia, and a daily bath of kerosene oil before going into the brush."

The author secured five hundred birds, one altogether new to Science; about a thousand eggs, many of them new or rare; a few mammals, all of which proved interesting; and quite a collection of insects.

Drs. Coues and Yarrow's "Notes on the Herpetology of Dakota and Montana" include a most valuable account of the rattlesnake, of which five species, belonging to the two closely-allied genera *Crotalus* and *Caudisoma*, infest the region in question. The authors do not believe in the alleged power of fascination possessed by this reptile, and consider that the use of the rattle is a problem still unsolved. "One thoroughly established fact concerning the rattle is that its practical operation is injurious to its possessor, by provoking attack from those who can cope with it successfully." There is no known specific for the poison of the rattlesnake, but its bite, unlike that of the cobra, is by no means invariably fatal.

Geological Survey of Victoria. No. 4. Report of Progress by the Secretary for Mines, with Reports on the Geology, Mineralogy, and Physical Structure of Various Parts of the Colony. By Messrs. Murray, Krause, Taylor, Howitt, Nicholas, McCoy, and Newberry. Melbourne: Ferres. London: Trübner and Co.

It is satisfactory to know that the exploration of Australia—geological, palæontological, and mineralogical—is not standing still. Attention is mainly directed, not unnaturally, to the occurrence of gold-fields and auriferous quartz veins. Still the existence of other useful minerals and the occurrence of animal and vegetable remains are not overlooked. Prof. McCoy makes

the following interesting remark:—"As in South America the Geological period, just before the creation of man, had the gigantic *Megatherium* to pre-figure the little sloths of the present day amongst the characteristic edentate group of mammals of the fauna of the same country, so the little native "bears" (*Phascolarctos*) of Victoria in our day were preceded, at the same late Tertiary period, by equally huge animals of their same general marsupial type, as characteristic of the existing Australian fauna as the edentate is of that of South America. The *Diprotodon* of Australia, curiously enough, like the *Megatherium* of South American deposits, was obviously a feeder on the twigs and foliage of trees, like their diminutive representatives of modern times."

Among the fossils figured is *Asterolepis ornata*, which occurs, though rarely, in the Middle Devonian Limestones of the Buchan River, and is almost identical with specimens found in the Russian Old Red Sandstone. Upon the occurrence of this species Prof. McCoy remarks:—"The great ganoid armour-plated fishes of the genus *Asterolepis* are amongst the most abundant and striking characteristics of the Devonian rocks of Russia, and it is certainly a most extraordinary circumstance to find them here in Australia in limestones of the same age, and accompanied by the corals and shells of the Plymouth and Eifel limestones of similar age, with which they are not known to occur in England or Germany, and which do not occur with them in the Russian beds."

As additional instances of the permanence of local type in Australia, both among plants and animals, may be cited the occurrence, in the Pliocene Tertiary argillaceous strata at Dayles-ford, of *Eucalyptus Pluti*, the foliage of which is in size and shape almost identical with that of the living *Eucalyptus globulus*. Relics of gigantic extinct kangaroos, *Macropus Titan* and *M. Atlas*, are found in the newer Pliocene Tertiary, 25 feet below the surface, at a place called Duck Ponds.

Several of the iron ores of Victoria have been analysed, and are considered valuable. Certain samples, from Lake Tyers and from Bonang, have been pronounced by an experienced English iron-master worth carriage from Melbourne into Shropshire. The reporter, however, thinks it doubtful whether even the best of the Victorian iron ores would, in their crude state, pay for export to England, owing to the expense of land-carriage to the sea-board.

The volume is illustrated with maps, diagrams, and plates, showing microscopical rock-sections, sections of strata, and views illustrative of the general character of the scenery.

Records of the Geological Survey of India. Vol. x., Parts 1, 2, 3. 1877.

The Annual Report of the Survey for the year 1876 contains some judicious strictures upon men of science who wish to force upon facts an interpretation favourable to their pet theories. The foreign relations of the Gondwana system are becoming, it seems, a burning question. "Palæontologists come from cabinets in Europe with the fixed idea that the 'laws' they have seen to work so well as between Bohemia and Bavaria, or from Durham to Dorsetshire, will apply equally between India and and Australia, or Europe, and the eager aim of their labours seems to be to tally off our Indian rock-groups as the representatives or equivalents of certain fossiliferous series in Europe, or elsewhere. From the beginning this palæontological fallacy has been the chief obstruction to our knowledge. When first the Gondwana fossils were taken, pure geology being in the ascendant, the fact that certain plant forms of the lower Gondwana rocks were somehow associated with beds having a carboniferous marine fauna in Australia was made the basis of a special pleading to show that the Damudas, their flora and their coal, were palæozoic. The materials have now come into the hands of a pure palæontologist. He has shown, I believe, conclusively that the Gondwana flora is wholly mesozoic, nailing its several phases to certain representative zones in Europe. But it so happens that on the confines of India, east and west, the upper Gondwana groups are associated with beds having a marine fauna, according to which these said groups have already been attached by palæontological experts to other standard groups in Europe. It is true that the study of this fauna was only partial, but the experts were very accomplished in their line, and their judgment was quite unprejudiced, so that it must carry great weight. Here then, again, is an opening for the procrustean method of research; and there are symptoms that it is to be duly applied, this time, to make the fauna conform to the flora. The expression 'palæontological contradiction,' which has been applied to this fact of association, exhibits the predicament in a very naive manner. The contradiction is certainly there, but only as a rebuke for those who can look upon it in that light. No theologian could be more impious in reducing the mysteries of existence to the compass of his narrow thoughts than are often scientific specialists in imposing crude conceptions upon the proceedings of Nature. Yet these ought to know better that truth is discovered, not invented."

Mr. W. T. Blanford reports on the Great Indian Desert, between Sind and Rajpootana, part of which at no remote date appears to have been an arm of the sea.

Dr. Feistmantel describes the occurrence of the cretaceous

genus *Omphalia* near Namcho Lake, in Tibet, and gives also a note on *Estheria* as occurring in the Gondwana formation.

Mr. Lydekker describes certain new and other vertebrates from Indian tertiary and secondary rocks, such as *Bos acutifrons* and *planifrons*, *Bubalus platyceros*, *Stegodon ganesa* (which elephantoid, like the allied *S. insignis*, lived down to the Nerbudda period, and must have been contemporary with the early human inhabitants of India), *Sivalhippus Theobaldi* (an aberrant horse from the Siwaliks), *Ictitherium Sivalense*, *Hyænarctos Sivalensis*, and certain Saurians, including the new genus *Titanosaurus*.

Part 2 comprises an account of the rocks of the Lower Godavari; notes on the Atgarh Sandstones near Cuttack; a paper on the Fossil Floras of India; a notice of some new or rare Mammals from the Siwaliks; a note on the Arvali Series in North-eastern Rajputana; and an account of some borings for coal, which describes the cost of the operation and the time consumed in perforating different rocks, but omits to state whether coal-beds were reached, and, if so, what were their thickness and quality.

There is subjoined a paper on the geology of India, which the late Dr. Waagen, formerly palæontologist to the Survey, had contributed to the "Zeitschrift der Deutschen Geolog. Gesellschaft. The author pronounces his opinion that the peninsula of India belonged to a great continent, which probably included China, the (Malayan) Archipelago, and Australia, perhaps even a part of Oceania.

Part 3 comprises "Notes on the Tertiary Zone and Underlying Rocks in the North-west Punjab," by A. B. Wynne, illustrated with a geological map of the district. In this region occur the celebrated salt deposits of the western frontier and certain petroleum springs of no great importance.

Dr. Feistmantel gives an account of a tree-fern stem from the cretaceous rocks near Trichinopoly.

Mr. W. Theobald treats on the occurrence of erratics, and concludes that at one time "glaciers were ploughing their way down the great Himalayan rivers and valleys, to within 2000 feet or so of the sea," whilst the Potwar was one great lake, with an exit probably near Kalabagh as now, and into which lake glaciers descended, freighted with the *débris* of the hills of Hazara and Kashmir.

Mr. F. R. Mallet gives an account of recent coal explorations in the Darjiling district. The coal found is extremely friable, and could not be worked without great expense, especially as the Assam coal could easily be brought to the foot of the Darjiling hills.

The same author likewise describes the limestones found at Barakar, and some blowing machines used by the smiths of Upper Assam.

The analyses of Raniganj coal, by Mr. A. Tween, show that the Indian samples are less contaminated with sulphur than the average of English specimens, but contain more ash and more oxygen. The Dumakunda and Benodakotta coals, however, have a good heating power, since, if the combustion of pure carbon yield 8080° C., they afford respectively 7040° and 7023°. The best gas-coal is that of Sanktoria, which furnishes about 9000 cubic feet per ton.

Memoirs of the Geological Survey of India. Vol. xii., Part 2. MALLET: Coal-fields of the Nágá Hills bordering the Lakhimpur. Calcutta: Geological Survey Office.

THE deposits of coal on the Brahmaputra and its affluents in Upper Assam are exceedingly valuable. The coal is of good quality, and nothing seems requisite save the improvement of the navigation of the river to bring it into use in Bengal, and supersede the necessity of importation from England. Petroleum is also found in quantity, but until the means of communication are improved it cannot compete in the market with that brought from Rangoon and from America. Some of the coal-beds are of very considerable thickness. Thus in a total section of 47 feet 10 inches the coal-beds alone amount to 37 feet, the thickest bed being 25 feet. It is an unfortunate circumstance, however, that the measures have a high general dip towards the hills, and the seams must rapidly sink to a depth below which the coal could not be profitably worked. Still the author estimates that as a minimum 9,000,000 tons of marketable coal would be easily procurable from the seams already known to exist. Between Tipam and Bornarchali there is a further minimum of say 10 millions of tons in a marketable state, whilst the Nazira field will add a further supply of 7 millions. In the unsettled state of the political horizon the existence of a supply of coal for naval purposes independent of importations from Britain is of the highest importance. Pyritous shales seem abundant, and the manufacture of copperas and alum could probably be conducted with success. The iron ore in the sub-Himalayan beds is inexhaustible, but the quality is very poor, and the rarity of limestone in the Nágá hills must always be a difficulty in the way of smelting operations on a large scale.

XUM

The Origin of the World, according to Revelation and Science. By J. W. Dawson, LL.D., F.R.S., &c. London: Hodder and Stoughton.

This work may be characterised as one of the many attempts to reconcile the doctrines of revealed religion with the results of modern science, or, as the author himself puts it, to "aid thoughtful men perplexed with the apparent antagonisms of science and religion, and to indicate how they may best harmonise our great and growing knowledge of Nature with our old and cherished beliefs as to the origin and destiny of man."

To those who hold fast the luminous principle laid down by Galileo, that on what may be called scientific subjects the Hebrew and Greek Scriptures speak merely the ideas of the times when they were written, and, though a moral, have no claim to be regarded as a physical revelation, such books as that before us present a curious and not altogether pleasing phenomenon. We may well ask whether Dr. Dawson and those who agree with him are not to a great extent responsible for the perplexity which they seek to remove? We deprecate as much as any one the gratuitous, and in our opinion unscientific, attacks upon religion to which such writers as Buchner have given way. But it is still very probable that much of this spirit springs from disgust at the attempts made to extract from Hebrew roots, under high-pressure philology, doctrines conformable with or anticipatory of the teachings of modern science.

To go through Dr. Dawson's treatise paragraph by paragraph would be a tedious and not very remunerative task, and could only be fairly undertaken by a writer well versed in theology, mythology, and philology.

It is very significant that we find the cosmogony of Laplace—the so-called nebular hypothesis—accepted, and proclaimed in harmony with the teachings of the Scriptures! But the other day, so to speak, this doctrine was denounced by Sir D. Brewster and others as an atheistical speculation, and an attempt to construct the universe without God. Just as Brewster opposed the nebular hypothesis, Dr. Dawson struggles against the doctrine of Evolution, each in turn insinuating that to throw a novel light on God's *modus operandi* is to deny His existence. The moral of this coincidence is too plain to be missed.

The author is, perhaps, in this work less magniloquent than in some of his former productions, but towards those who do not see what he sees—or rather who see what he cannot, or will not—he is scantily courteous. "Shallowness," "mere folly and presumption," are the attributes which he finds in all dissentients.

Pollen. By M. P. Edgeworth, F.L.S., F.A.S. Illustrated with 438 Figures. London: Hardwicke and Bogue.

This interesting monograph was originally laid before the Linnæan Society; circumstances, however, determined the author to withdraw the paper, and it now appears in the present altered form. The introduction explains the varied forms which pollen takes in different plants, and gives the bibliography of the subject. This is followed by a list of plants the pollen of which has been examined, with references to authors who have made the observations; the species examined by the author are marked with an asterisk, and form a considerable part of the catalogue. This valuable portion of the work occupies sixty pages. The most important part of the book, however, consists of the twenty-four plates, containing four hundred and thirty-eight figures, and in this, with the explanation, is contained the result of the author's own microscopical observations. Many of the pollens are figured as they appear when viewed under different conditions, *e.g.*, dry or opaque objects or in various fluid media, as water, oil, vinegar, &c. The author has not put his own drawings on the stone, but they appear to have been rendered with tolerable fidelity by the lithographer. It is to be regretted that so few observers will take the small amount of trouble needed to acquire the various lithographic processes, and prevent the risk of misinterpretation so liable to occur when drawings are copied by those who, however skilled they may be in the technicalities of their art, can hardly be expected to understand the nature of unfamiliar objects.

The drawings are all to scale (1-500th of life size), a matter too often neglected, and when this is the case detracting from the value of otherwise good figures; for rigid adherence to this principle the author is highly to be commended, as the comparative dimensions of any pollen can be readily estimated.

Mr. Edgeworth's monograph will prove of great value to the student of this department of minute vegetable structure.

CORRESPONDENCE.

RESIDUAL PHENOMENA.

To the Editor of the QUARTERLY JOURNAL OF SCIENCE.

SIR,—In the article on "Residual Phenomena" published in the last issue of your Journal it is stated that the actual atomic heats of carbon, boron, and silicon are much lower than those required by the law of Dulong and Petit, and it is assumed that there is some residual phenomenon modifying the result in these three instances. It is also stated that "though the anomalies to which we have drawn attention have been recognised for more than a quarter of a century, the explanation is still wanting."

I would draw the author's attention to the researches of F. Weber respecting the specific heats of the three above-mentioned elements (Ann. Chim. Phys. [5], viii.; abst. Journ. Chem. Soc., June, 1876). Weber found that the specific heats of these bodies increase *rapidly* with the temperature, and at high temperatures tend to become constant. If we multiply the specific heats at these high temperatures into the atomic weights, we obtain the atomic heats. Carbon—(1) diamond, 5·5; (2) graphite, 5·6; silicon, 5·75; boron, 5·5. Thus it appears that the true atomic heats of these three elements do not vary from the mean number 6·4 to a greater extent than those of other elements of low combining weight, such as aluminium.

Here, then, we have the residual phenomenon alluded to; and it is interesting to note that its explanation affords a further confirmation of Dulong and Petit's law.—I am, &c.,

C. H. BOTHAMLEY.

The Yorkshire College,
February 20, 1878.

SCIENTIFIC NOTES.

CONTINUING his researches on the repulsive force resulting from radiation transmitted through rarefied gases, Mr. Crookes constructed a torsion balance in which the beam for carrying the experimental disks was a straw suspended by a very fine glass fibre. His experiments extended over one hundred different substances, and he measured the effect produced by them, not only under the influence of simple radiation from a luminous source, but also by removing the invisible heat rays by the interposition of a water screen. In this manner he found that each substance exercises a more or less distinct action on the absorption of rays. Thus most white powders powerfully absorb the invisible heat rays, while they are almost without action on the luminous rays. On the contrary, black powders powerfully absorb the luminous rays, and only slightly absorb the obscure heat rays, whatever their intensity may be. The different metals present great differences in their action. Iron, for instance, chiefly absorbs the invisible heat rays, while gold is principally acted on by the luminous rays. The substances experimented with may be divided into two classes: 1st. Those whose action is increased by the interposition of water screens with regard to the effect produced on the standard disk; 2nd. Those in which the contrary is the case. Amongst the former may be mentioned copper tungstate, saffranin, precipitated selenium, and copper oxalate; these are more affected by light than by invisible heat. Amonst class 2 may be mentioned chromic oxide, persulphocyanogen, zinc oxide, barium sulphate, and calcium carbonate; these are acted upon more by the ultra-red rays than by the luminous rays. Remarkable effects were also obtained by combining the substances in these two categories on the disk of the radiometer. Mr. Crookes has previously shown that when the exhaustion of a radiometer is carried beyond a certain limit its sensibility gradually diminishes until it becomes absolutely null. He has now come to the conclusion —1st. That there is a gradual increase in the sensitiveness of the radiometer until the pressure has attained 50 millionths of an atmosphere (0·038 millim.); 2nd. That beyond this limit to 30 millionths of an atmosphere (0·013 millim.) it remains stationary; 3rd. Further still, it sinks rapidly until at 1 millionth (0·00076 millim.); 4th. That at 0·2 millionth of an atmosphere (0·00015 millim.) the radiometer refuses to turn even when five candles are put near it. He has also examined the effects of molecular pressure produced directly by heat upon a radiometer with the following results:—(1.) When the apparatus is full of air at the normal pressure of 760 m.m., and a platinum ring was rendered incandescent by an electric current, the direction of the rotation of the vanes and disk was *positive*,—that is to say, that which would be produced by a current of air coming from the platinum ring; this effect must be attributed to the ascending current of hot air. (2.) At a pressure of 80 millims. the disk did not turn. The vanes turned slowly in the positive direction. (3.) At 19 millims no movement was produced either by the disk or by the vanes. (4.) At 14 millims. the disk remained stationary, but the vanes began to turn gently in the negative direction,—that is to say, in a way inverse to their first direction. (5.) At 1 millim. the disk turned in a continuous manner in the positive direction, whilst the vanes turned rather fast in a negative direction. (6.) At 0·224 millim. the speed of the disk or the vanes was the same, and their rotative movement was the same. Below that pressure the speed of the rotation of the vanes diminished gradually, whilst the speed of the disk increased, and at a pressure of 0·107 millim. the disk turned rapidly in the positive direction, whilst the vanes were motionless. (7.) With a more perfect exhaustion still, at 0·098 millim. a sudden change was seen. The

vanes which had been stationary then began to turn in the positive direction at a speed of 100 revolutions per minute, whilst the disks turned as before, positively, but with less speed. (8.) When the exhaustion was carried beyond 0·098 millim. the speed of the two disks and of the vanes increased till it exceeded 600 revolutions per minute, and it did not seem to diminish with the highest rarefaction, which was at 0·0001 millim. According to the most recent determinations the number of molecules contained in a cubic centimetre of air at the ordinary pressure is probably something like—

1,000,000,000,000,000,000,000 (one thousand trillions);

consequently, at an exhaustion of 0·0001 millim., 100,000,000,000,000 are still left. This number is sufficiently large to justify the hypothesis—That when the molecules are set in vibration by a white-hot platinum wire they are still capable of exercising an enormous mechanical effect.

Mr. J. W. Groves, of the South London Microscopical Club, after cleaning glass slides for mounting microscopical objects, by one of the usual processes, fastens them together by their edges, after the manner of the well-known artists' sketching-blocks. This is easily done with a pile of slips, by fixing round their edges a piece of ready-gummed tissue-paper, 10 inches long, and of a width suitable to the number of slides, so that, although they are firmly bound together, their surfaces are left uncovered. The block is left to dry, when each slip may be detached by running the thumb-nail round its edges. The surface next the adjoining slip should be used for the preparation to be mounted on, as it is, of course, quite clean, although the exposed one may have become dirty: the fragments of tissue-paper are removed after the mount is completed.

Mr. H. C. Sorby, F.R.S., has described to the Royal Microscopical Society a new arrangement for distinguishing the direction of the axes of doubly-refracting substances. The usual method is to employ plates of selenite of various thickness: this, however, involves great difficulty in selecting one of nearly the same tint as that of the crystal examined under the polarising microscope; and unless this can be done it is impossible to obtain so decided a result that the direction of the positive and negative axes can be seen at once and no consideration be required. Mr. Sorby employs a wedge-shaped plate of quartz, cut parallel to the principal axis, 1¼ inches long and ½ an inch wide. At its thickest end it is 1-20th of an inch thick, and thins off to the sharpest possible edge: this is fixed on a glass plate so as to leave a space of glass 4-10ths of an inch long by ½ an inch broad beyond this thin end of the quartz. The combined plates are fixed in a brass frame, like that for a micrometer, which slides into the eye-piece. On using polarised light with a crossed analyser over the eye-piece, and arranging the plate so that the part with only glass is in front, we see the object in its normal state, and the rest of the field black, and on pushing forward the quartz wedge we see the field of the microscope crossed with coloured bands, gradually rising from the bluish white of the first order, through all the brighter orders of colours, to the faint reds and greens, and upwards to what cannot be distinguished by the unaided eye from white light. If some crystal, giving any tint, be on the stage of the microscope, we can usually see at once whether the tints are raised or depressed, by the manner in which it alters the colour of the bands; and by pushing the quartz wedge backwards and forwards, there may be no difficulty in finding the exact place where the plate of quartz so exactly neutralises the action of the crystal that it appears black. If this does not occur in any place, and, on the contrary, the tints appear to be raised, the eye-piece and the plate must be rotated through an angle of 90 degrees, and the requisite place can then be easily found. The plate of quartz being so cut that its longer axis is parallel to the principal axis of the crystal, we know that this longer axis is positive, and thus also at once know which is the positive and which the negative axis of the crystal under examination. We can also at once see what is the true order of colour which it gives, since we can readily count it up from the bands due to the quartz alone seen crossing the field of

the microscope. We are also by no means limited to visible tints. The crystal may have such a powerful double refraction, or be so thick as to give apparently white light, and yet, by using the thicker end of the quartz wedge, the tints may be reduced down to those easily distinguished. This simple arrangement secures all the advantages of an unmanageably large number of selenite plates, and all necessary observations can be made with ease and expedition.

A valuable paper on the "Application of the Micro-Spectroscope to the Study of Evergreens" was read before the Royal Microscopical Society (November 7th, 1877), by Thomas Palmer, B.Sc. As it would be impossible to give an abstract that would be of any use to the student, and, moreover, the reproduction of Mr. Palmer's spectrum charts would be required, reference is made to the original paper.

Owing to the death of Dr. Henry Lawson, F.R.M.S., and Assistant-Physician and Lecturer on Physiology at St. Mary's Hospital, and formerly editor of the "Monthly Microscopical Journal," that publication will be discontinued. The "Transactions of the Royal Microscopical Society" will for the future be issued by themselves, after the manner of the larger societies.

A mode of examining water microscopically has been contrived by W. L. Scott, Public Analyst to the County of Glamorgan and Borough of Hanley. The chief point in the process consists in the manner of filtering the water, by which the organisms contained in a large quantity of material are retained on a very small portion of the filter-paper. The centre of the filter is rendered impervious by means of a fatty composition, and the texture of the paper rendered more obstructive to the passage of minute organisms by being dipped in a very thin structureless collodion. The process is described in detail in the "Monthly Microscopical Journal" (vol. xviii., p. 239).

In the obituary notices of the Royal Microscopical Society we remark the death of Mr. J. L. Denman. Although not distinguished for any published work in the department of minute structure, he has done that which will endear his memory to every analyst. An honest tradesman, disgusted with the adulteration practised—and unfortunately approved of by the public—in the article in which he dealt, he devoted much time and expended considerable capital in obtaining pure wine. Failing to obtain it from the usual sources, he sought it in other countries, and, in spite of popular prejudice, imported the unadulterated wines of Greece and Hungary. Just before his death he had the satisfaction of introducing pure wine from Spain—a country which, like Portugal, had always prepared its exports to the vitiated taste of the British consumer. His definition of wine was simply "fermented grape-juice," in old times coupled with bread as a necessary of life.

The Telephone continues to engage the attention of physicists. A very complete and concise description of the construction and action of the instruments of Reiss, Yeates, Gray, Edison, and Bell is contained in the report of a Lecture by Prof. Barrett, of the Royal College of Science, Dublin, and published by Mr. Yeates, of King Street, Covent Garden.

In a paper "On some Physical Points connected with the Telephone," read before the Physical Society, Mr. W. H. Preece has pointed out that this instrument may be employed both as a source of a new kind of current and as the detector of currents which are incapable of influencing the galvanometer. It shows that the form and duration of Faraday's magneto-electric currents are dependent on the rate and duration of motion of the lines of force producing them, and that the currents produced by the alteration of a magnetic field vary in strength with the rate of alteration of that field; and further, that the infinitely small and possibly only molecular movement of the iron plate is sufficient to occasion the requisite motion of the lines of force. He also pointed out that the telephone explodes the notion that iron takes time to be magnetised and de-magnetised. The instrument forms an excellent detector in a Wheatstone bridge for testing short lengths of wire, and condensers can be adjusted by its means with great accuracy. M. Niaudet has shown, by employing a doubly wound coil, that it can be used to detect cur-

rents from doubtful sources of electricity, and it is excellent as a means of testing leaky insulators. Among the facts already proved by the telephone may be mentioned the existence of currents due to induction in wires contiguous to wires carrying currents, even when these are near each other for only a short distance. Mr. Preece finds that if the telephone wire be enclosed in a conducting sheath which is in connection with the earth, all effects of electric induction are avoided; and, further, if the sheath be of iron, magnetic induction also is avoided, and the telephone acts perfectly. It appears that conversation can be carried on through 100 miles of submarine cable, or 200 miles of a single wire, without difficulty, with the instrument as now constructed; but the leakage occurring on pole-lines is fatal to its use in wet weather for distances beyond 5 miles.

M. Demoget, of Nantes, has experimented with two Bell telephones in an open field. He held one of these to his ear, while his son at a distance kept repeating the same syllable with the same intensity of voice into the second instrument. He compared in this way the sound heard from the telephone with that heard from the speaker, and calculated their relative intensities from the relative distances of their sources. From the results M. Demoget concludes that the telephone as a machine leaves much to be desired, since it can only transmit 1-1800th of the original work.

Dr. R. M. Ferguson has contributed some valuable "Notes on the Telephone" to the Royal Scottish Society of Arts. He takes exception to the vibration theory of Bell, viz., that it is the vibrations of the disk to and from the pole of the magnet, in excursions proportionate to the intensity, pitch, and quality of the vocal sounds, that electrically affect the instrument. He submits that at the receiving station it can be proved well nigh to demonstration that it is a molecular tremor or vibration, and not a vibration mechanically produced, that emits the sound; and that this molecular vibration becomes louder the easier the sounding body vibrates. Seeing that there is the most perfect correspondence between the sending and receiving instruments, there is, he says, every reason to believe that the sending instrument exhibits the converse action to the receiving instrument. and that there again sound acts on iron so as to produce molecular changes, the electric power of which is much enhanced by the vibration of the sounding body.

The Molecular Theory of the Telephone has also been the subject of a paper at the King's College Engineering Society, by Mr. C. W. Cunnington, who considers that the vibration of the iron disk of the sending instrument under the influence of sound is amply sufficient to induce currents of electricity strong enough to cause the plate of the receiving instrument to vibrate, and thus to reproduce sound: he also referred some of the sound produced in the receiving instrument to the effect of the undulatory currents on the magnet itself, thus explaining results obtained without the use of a vibrating disk. In comparing the vibrating disk of the telephone with the membrana tympani of the ear, Mr. Cunnington pointed out the fact that the predominance of the fundamental note of a flat plate would drown a large number of the overtones of the voice, thus causing many of the observed peculiarities of the sound transmitted in ordinary telephones; the membrana tympani being funnel-shaped, whilst peculiarly susceptible to the influence of sound, had no fundamental note of its own, and therefore transmitted all of the sound vibrations impinging upon it (within certain limits) without giving undue preponderance to any particular note.

The Count du Moncel finds that the vibratory plate of the recipient cannot merely be replaced by a very thick and massive armature without affecting the transmission of speech, but these vibratory plates may be formed of non-magnetic substances. The vibratory plate may even be totally suppressed without hindering the telephonic transmission provided the polar extremity of the magnet is placed close to the ear. Hence the vibrations which reproduce speech in the receiving telephone are principally produced by the metallic nucleus infolded by the coil. The vibratory plate serves merely to react for the production of induced currents, when set in vibration by the voice, and by its reaction upon the polar extremity of the magnetic rod to reinforce the magnetic effects produced by the latter.

At a meeting of the Physical Society, in February last, Mr. F. J. M. Page exhibited the action of the telephone on a capillary electrometer. He first explained the construction of Lippmann's electrometer, as modified by Marey, and threw the meniscus of the mercury in the capillary tube on the screen by the electric light. The delicacy of the instrument was shown by passing a current of 1-1000th of a Daniell, which caused a distinct movement of the mercury. Resistances of 500 ohms and 1-50th ohm gave approximately the same deflection, so that, in practice, the instrument may be considered to be independent of resistance, in addition to which it possesses the great advantage of portability, and its indications are almost instantaneous. To illustrate the use of the electrometer for physiological investigations, a frog's heart was connected by non-polarisable electrodes with the instrument: each beat of the heart caused a considerable movement of the mercury column. A telephone was now connected. On pressing in the iron plate the mercury moved, and on reversing the wires the movement was seen to be in the opposite direction. On singing to the telephone each note produced a movement; but the fundamental note of the plate, as well as its octaves and fifths, had the greatest effect. On speaking, the mercury oscillated continually: some letters of the alphabet had scarcely any effect, and the *w* was especially curious, producing a double movement. Reversing the wires did not alter the character or *direction* of these movements. The same effect was observed when the telephone was in the primary and the electrometer in the secondary coil of a Du Bois Reymond's induction coil. In conclusion, Mr. Page showed the contractions produced in a frog's leg. On inserting under the sciatic nerve two platinum wires coupled with the binding-screws of a telephone, and talking to this instrument, violent contractions ensued. In the course of the discussion which followed, Prof. Graham Bell said he had made very many attempts to ascertain the strength of the current produced by the human voice in vain; he considered, however, that the present method will in all probability give some most valuable results.

On Thursday evening, January 10th, M. Raoul Pictet succeeded in liquefying hydrogen gas in the laboratories of the Society for the Construction of Physical Instruments, at Plainpalais. The experiment, which was performed in the presence of several people, succeeded perfectly. The process consists in decomposing formiate of potash by caustic potash, a reaction which, as proved by M. Berthelot, gives hydrogen absolutely pure. The pressure commenced to rise at half-past eight; gradually and without any stoppage it attained, at seven minutes past nine, 650 atmospheres, at which point it remained steady for a few instants. At this moment the tap was opened, and a steel-blue jet escaped from the orifice, producing a hissing sound like a bar of red-hot iron plunged into water. The jet suddenly became intermittent, and there could be observed a hail of solid particles projected violently to the ground, on which their fall produced a crackling noise. The tap was closed, and the pressure, which was then at 370 atmospheres, gradually descended to 320, where it remained for some minutes. It then rose to 325. At this moment the tap, on being opened a second time, only allowed a jet to escape intermittently, rendering it evident that a crystallisation had taken place in the interior of the tube. The proof of this can be established by the escape of hydrogen in the liquid state when the temperature begins to rise on stopping the pumps. Thus has been experimentally demonstrated the liquefaction, and especially the solidification, of this gas, which from its properties has always been considered probably to belong to the class of metals.

M. Cailletet sent a paper on the "Liquefaction of Nitrogen, Hydrogen, and Atmospheric Air" to the Academie des Sciences, which was read at the meeting on the 31st of December, 1877. M. Cailletet showed that pure and dry nitrogen compressed to about 200 atmospheres, at a temperature of $+13^{\circ}$, then allowed to expand suddenly, condenses in the most perfect manner: it first produces an appearance like that of a pulverised liquid in small drops of appreciable volume; this liquid then gradually disappears from the sides to the centre of the tube, at last forming a sort of vertical column following the axis of the tube. The duration of these phenomena is about 3 seconds. On

experimenting with hydrogen—in the presence of MM. Berthelot, H. Sainte-Claire Deville, and Mascart—M. Cailletet succeeded in observing indications of its liquefaction under conditions of proof which left no doubt in the minds of the scientific men who witnessed the experiment. It was repeated a great number of times. Operating with pure hydrogen compressed to about 280 atmospheres, and then suddenly allowed to expand, they saw form an extremely attenuated and subtle mist suspended in the gas and disappearing suddenly. Having liquefied nitrogen and oxygen, the liquefaction of air is thereby demonstrated. It appeared, however, of interest to make this the subject of an actual experiment, and, as might be expected, it succeeded perfectly. I need not say that the air was previously dried and freed from carbonic acid. The accuracy of the views expressed by the founder of modern chemistry, Lavoisier, is thus confirmed as to the possibility of causing air to assume the liquid state, and of producing matter gifted with new and unknown properties.

At a recent meeting of the Académie des Sciences M. Lecoq de Boisbaudran exhibited a bar, a sheet, and several crystals of the new metal, *Gallium*, which is harder than iron, yet melts at under the heat of the finger, its freezing-point being at about 30°. It is proposed to use it for a thermometer going up to red-heat. 5000 kilogrms. of ore had to be worked down to get 60 grms. of metal. It will now be possible to investigate its physical properties. It adheres to glass, and is very brittle; the colour is nearly that of steel; and the crystals are octahedra. M. Lecoq de Boisbaudran has since determined the atomic weight of gallium; it is 69·9.

A photo-lithographic plat of the primary triangulation carried on during the summer of 1877, by Mr. A. D. Wilson, Chief Topographer, has just been published by the U.S. Geological Survey, under the charge of Dr. F. V. Hayden. The area covered by these triangles extends from Fort Steele, in Wyoming, *Zy.*, westward to Ogden, in Utah, *Zy.*, a distance of about 260 miles, and north as far as the Grand Teton, near the Yellowstone National Park, including Fremont's Peak, of the Wind River Range of the Rocky Mountains. The area embraces about 28,000 square miles, and within it twenty-six primary stations were occupied and their positions accurately computed. Besides these occupied stations a large number of mountain peaks were located, which in the future will be occupied as points for the extension of the topographical work of the Survey. A base line was carefully measured near Rawlins' Springs, on the line of the Union Pacific R.R., and from this initial base the work was extended north and west to the valley of Bear River, in Idaho, *Zy.* Here a check base was measured, and the system expanded to the neighbouring mountain peaks, to connect with the triangulation, as brought forward from the first-mentioned base. Along the line of the Union Pacific R.R. the work was connected at six points with the Triangulation System of Clarence King's 40th Parallel Survey. In addition to the importance of this sheet as the base of the season's topographical work, it presents a most striking feature in the number of remarkably long sights which were taken from some of the most lofty mountains in the area explored. Many of these sights were over 100 miles in length, while some reach a distance of 135 miles. From Wind River Peak all the prominent points in the Big Horn Mountain were sighted; also the loftier peaks of the Uinta Mountains: the former are located 165 miles to the north-east, while the Uinta Mountains are situated about the same distance to the south-west. As these ranges were not in the scope of the season's work they are not given on the chart.